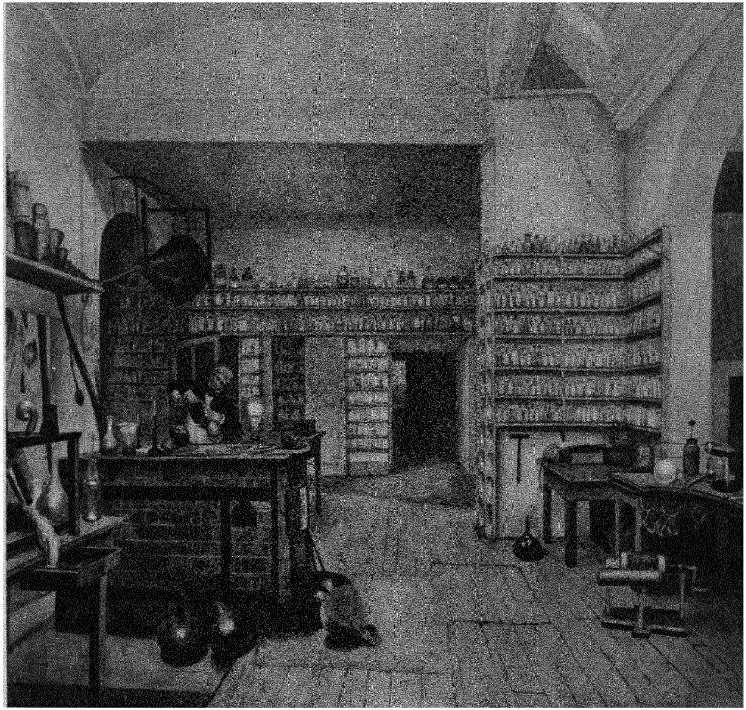
FARADAY'S DIARY

VOL. V



FARADAY IN HIS LABORATORY AT THE ROYAL INSTITUTION, 1852

From a water-colour drawing by Harriet Moore, in the possession of the Institution

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY

D.C.L., ~~F.R.S.~~

during the years 1820–1862
and bequeathed by him to the
ROYAL INSTITUTION OF GREAT BRITAIN
Now, by order of the Managers,
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SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. V

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Faraday in his laboratory at the Royal Institution, 1852 *Frontispiece*

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FOLIO VOLUME V OF MANUSCRIPT

(CONTINUED)

SEPTR. 6, 1847.

9022. Have arranged a lime light, using oxygen and hydrogen, and have arranged to gather a good bundle of the rays by means of convex glasses, so as to give a good ray which could be sent through a helix of 500 feet of wire, either free or between the magnetic poles. The Iron poles were pierced so that the ray pass in the middle of the magnetic field.

9023. Have also arranged an interceptor, which could cut off the ray and then let it pass again, and which by its connexions with the helix through which the ray had to pass and also with the galvanometer, could intercept and renew the circuit—so that the circuit could be completed either as the rays began to pass through the helix and be interrupted when the rays were cut off—or vice versa, and that with great rapidity, so as to have very many alternations in a minute or even in a second. Perhaps 400 or 500 in a second—quite able for that. 1 revolution of great wheel = 360 interceptions.

9024. On proceeding to experiment, it was found that a more perfect insulation of the wires and helix proceeding to the galvanometer must be made from the Magnet and battery before the experiment could be made in the magnetic field. Otherwise the arrangement was good.

9025. The lime light ray in its common state was sent through the helix with air in it—also with heavy glass—prism of rock crystal—and a tourmaline with one side cut away parallel to the axis—but in no case was there any effect produced at the Galvanometer. The circuit was in all these cases ascertained to be perfect for *each* experiment by trial with a thermo electric circuit.

9026. The ray was polarized by a Nicol eye piece—but still no effect.

9027. The ray was in all these cases intercepted by the instrument, but neither at the rising of the ray or ceasing of the ray in the helix and media was any thing produced.

SEPTR. 7, 1847.

9028. Have had the interceptor, helix and parts included in the galvanometer circuit insulated by ivory attachments, so as to preclude derived currents. Yet when all in place and the battery

on to the magnet, there was an effect at the galvanometer—the needle joggled about to the right and the left, 10° or 15°, and came to a stand suddenly and quickly changed its direction, as if there were a series of irregular feeble currents generated in the helix between the poles. By further insulation of the wire and parts, I am almost sure these were due to variations in the magnetism of the great magnet, and these perhaps due to some variations in the intensity of the current of the battery, caused perhaps by the fluid in the cells, etc. I cannot say distinctly. Sometimes these effects at the galvanometer were more and sometimes less. Allowance was made for these as well as could be in the following experiments.

9029. *Clear heavy glass* in the helix of 500 feet of wire (9022) either in the magnetic field or out—with the lime light (9022) on or off—ray intercepted or not (9023)—polarized or not. Gave no signs of an electric current under any of these variations by the influence of light.

9030. The dark green solution of proto ammonio tartrate of Iron (9037) was put into a tube with flat plate ends and that into the same core—it was so dark that no portion of the light appeared through it. Yet whether in or out of the magnetic field—with common or polarized light—continuous or intermitted—beging. or ending ray—there was no sign of a current due to the action of light.

9031. A prism of *dark quartz* in a similar helix under all the above variations gave no result.

9032. A *piece of heavy glass*—dark from reduced particles of lead within—under the same circumstances gave nothing.

9033. I think that I may trust the reality of these negative results, notwithstanding the small variations of the galvanometer (9028). Perhaps it might be worth while to try the sun's ray some fine summer day with the heavy glass and the Tartaric rotating solution—and then also use either Violet or red ray alone.

Reflexion of light from a wire.

9034. Some fine flat silvered copper wire, such as is used for whip handles, etc., was procured and 430 feet of it was wound round a flat smooth board about a foot square, in separate lines, resembling a deformed helix of which the strands or part were

out of contact with each other. The ends of this wire were then connected with a delicate galvanometer, and one of the faces of the board exposed to the sun's rays. In this way, about 210 feet of the wire could be submitted to the rays either in a vertical or oblique position, and with the rays along or across the wire, but no trace of any electric current due to the act of reflecting the light could be in any case perceived.

9035. Then the silver wire on one side the board was browned by a solution of sulphuret of potash, so as to cause absorption of great part of the light—but still no trace of any current produced by the act of absorption could be perceived.

9036. Still, I think, there must be some relation between these functions of light and electric forces.

9 SEPTR. 1847.

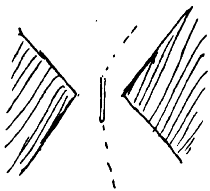
9037. When Tartaric acid and water are made to act for a few days on iron filings—and then ammonia in excess is added till resolution of the precipitate occurs—a dark green solution is procured—which is a good absorber of light—is magnetic—and probably rotative. By leaving this solution exposed to air, it became red brown at the surface as the iron peroxidizes and this action after some days extends to the bottom of the liquor.

15 OCTR. 1847.

9038. Experiments on the reflection of light by a flat wire carrying a strong voltaic current in the Magnetic field.

9039. It did not seem impossible or improbable that whilst a metal carried an electric current, its condition might influence the reflective force of the particles at its surface. A platina wire, fine, was therefore rolled flat and soldered on to ends of two thick and bent copper wires in such a manner that, whilst the latter being fixed firmly in a block of wood supported the platina wire, the platinum wire could be placed in any direction between the magnetic poles. The platinum wire was about half an inch long, and was heated by a battery of 5 pr. Grove's plates, which could easily raise it to whiteness if needed.

9040. The Electro magnet was served by 10 pr. Grove's plates and was very powerful, being the large Magnet with poles placed

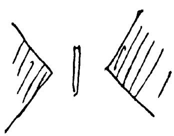


anglewise so to increase the forces. In the first instance, the platina wire was placed across the magnetic axis and then an ordinary ray of light sent down upon it from a lamp and the reflected ray observed either by the naked eye or by a Nicoll's Eye piece. But whether the magnet was on alone or the Electric current through the wire alone, or both on together, no effect on the ray could be observed, either whilst the actions were continued or intermitting, or at the beginnings or endings.

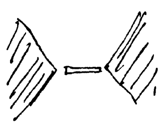
9041. In the first case the ray was sent in a vertical plane parallel to the wire and the incidences were varied from high to low angles—but there was no effect in any case.

9042. Then the incident ray was polarized by a second Nicol's eye piece and the reflected ray observed in several positions of the examining eye piece—but no effect under any combination of the magnetic and electric force was produced.

9043. The ray was polarized in a vertical plane and also in a plane at right angles to this (the ray *always* being in a vertical plane parallel to the wire) but still there was no effect.



9044. Then the wire and the magnet, being still in the same position, the *ray* was made to pass in a plane transverse to the wire and consequently parallel to the magnetic axis—and all the former variations of the ray were gone through, but without producing any sensible new phenomena.



9045. After this the wire was placed in the Magnetic axis and the ray sent parallel to it, but no effect could be produced under all the former variations of the experiment.

9046. Lastly, the wire being parallel to the Magnetic axis, the ray was sent across it, or transverse to the axis—but without any sensible effect on it.

9047. The light from the wire itself (when heated enough) was observed under all these conditions, but in no case did it seem affected.

9048. Whenever the Magnet were first made and then the electric current sent through the wire, there was a sharp small metallic sound from the Mag. field, but it was difficult to say whether it came from the wire or from the poles of the magnet. It was manifestly due to their joint action, for making the magnet active alone, or sending the electric current thrgh. the wire alone, did

not produce it. If the current was sent thrgh. first and *then* the Magnet made effective, there was *no* sound—but probably because the magnetic force is not generated suddenly as the electric current is, and suddenness is doubtless necessary to the *sensible sound* (9099).

9049. I removed the fine platina wire and replaced it by a moderate wire, and also a coarser wire, but in neither case was there any of the peculiar sound—as if the fine wire or concentrated current was far the most effectual.

9050. The magnet action being continued, there was the sound both on *making* and on *breaking* contact—and the latter is the most distinct.

9051. The phenomenon is the same, I conclude, as that which De la Rive has described.

9052. The sound occurred whether the fine platina wire was transverse or parallel to the magnetic axis.

23 OCTR. 1847.

9053. As to reflexion of ray, arranged a glass basin with a glass rod across it from side to side, so as to divide it into two places, and then put in a mixture of 1 vol. Sulc. acid and 4 vols. water until it just covered the glass rod. In this case there were 2 masses of Electrolyte connected by a thin portion covering the rod. Two platinum plates were in the masses, to be connected at pleasure with a battery of 10 pr. of Grove's plates. A wrapper of bibulous paper was put round the Glass rod, so as to keep the part of the fluid within it clear from bubbles arising from the plates; and reflexion of the ray was effected at the part over the rod, where the Electrolyte was thinned and consequently the current most powerful.

9054. Above, *a* is the basin, *b* the rod seen in section, *c* the bibulous paper, *d*, *d* the electrolyte, *e*, *e* the two electrodes, and *f* the ray.

9055. Now whether this ray were at the polarizing angle with the surface of the fluid—or above or below it considerably—whether it were a common ray or a polarized ray—and whether examined by the naked eye or an analyser, *no difference of effect* could be

observed when the electric current was passing the place of reflexion or when it was not.

9056. The terminations of the Magnetic poles of great Electro magnet were made polished iron cones, and these being brought very near together, a drop of water was placed between them; but whether the magnetism was on or off, no motion of the drop could be seen—no sensible effect of any kind took place.

9057. When the drop was a solution of iron or cobalt, then on putting on the magnetic force the drop was drawn into shape thus, so as to connect the magnetic poles in the most favourable manner.

9058. The cone poles were put very close to each other and a fine iron particle between; then a spirit lamp flame was applied to heat it whilst the magnetism was on, but no particular or new effect was observable of any kind.

9059. A platina particle held there and heated under the same circumstances—no difference whether magnetism was on or off.

9060. *Rose's Metals.*

H. Rose has sent me some pure specimens of the metals *Aluminium*, *Berylium*, *Tantalum*, *Niobium*, *Pelopium*[1], and I have just tried the magnetic conditions of them, putting them into a little glass tube suspended by a long thread before a single pole of the Electro magnet.

The little glass tube is very slightly diamagnetic, being very thin glass.

9061. *Aluminium*. This is very slightly magnetic.

9062. *Berylium*. Is also magnetic—not much but more than aluminium. A very small proportion of iron would give this degree of force.

9063. *Tantalum*—not magnetic—feebly diamagnetic and, I think, if any thing, less than the vessel alone—so that it is probably at zero.

9064. *Niobium*—as Tantalum.

9065. *Pelopium*—as Tantalum.

[1] Name given to a supposed new metal found in the mineral tantalite, afterwards discovered to be identical with niobium.—*O.E.D.*

9066. Proceeded to verify Zantedeschi's account of the diamagnetic condition of flame and instantly succeeded. Thus—

9067. Using a white wax taper flame about $1\frac{1}{2}$ inches long, and the two magnetic poles of this form, when they were about as in the figure* and the flame so placed as to be *either beyond* or *on this side* of the magnetic axis, and the latter about $\frac{1}{3}$ the height of the flame from the top; the moment the magnet was made effectual, the flame was affected and inclined outwards, being *repelled* from the axis and shewing itself *diamagnetic*.

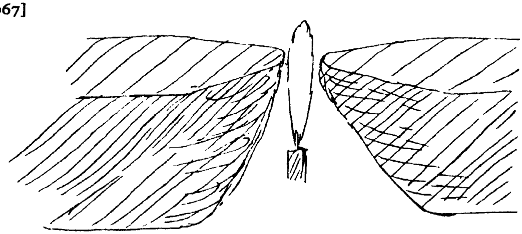
9068. The effect was not instantaneous but rose gradually to a maximum, and ceased instantly or much more quickly when magnetism was taken off. This was due to the graduall arrangement of the currents in the air, which tended to be formed upon the assumption of the magnetic condition.

9069. When the flame was placed at first across the magnetic axis, then the magnetism also affected it. The effect was to compress the flame between the points of the poles, making it receed in those parts from the poles towards the middle transverse line; also to shorten the top of the flame, and to make that and the sides of the compressed part burn more vividly, as if a stream of air set in from the poles on each side directly against the flame and passed out with it in equatorial direction; and such streams were really produced. But there was, at the same time, a repulsion of the parts of the flame from the axial line, for those parts which were below it and therefore ascending towards it, ascended with less force when the magnetism was on, and instead of continuing to go right up between the poles, tended to pass off to the right and left.

9070. On raising the flame a little more, the effect of the magnetic force was to increase the intensity of the results just described, and the flame actually became of a fish tail shape, disposed across the magnetic axis—shewing in a striking manner the diamagnetic condition of it as a whole.

9071. If whilst the flame was in this condition the Magnetic poles were brought a little nearer, so as to begin to cool and compress the part of the flame between them, and yet not prevent its rising

* [9067]



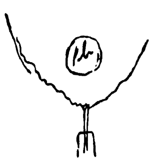
up there freely (which happens when they are about the third of an inch apart), then on making the magnet active the flame was more compressed and shortened than before; and as the effects proceeded towards the maximum state, the top at last went down, and the flame no more rose between the Mag. poles, but spread out right and left, and being made very bright along the upper extended forked edge by a current of air, which *descended* from between the poles on to the flame—driving it down and passing out and away in the equatorial direction.

9072. As soon as the Magnet was thrown out of action, the flame resumed its ordinary form *instantly*, and rose up between the poles—but the moment the Electric current was put on to the magnet, the flame sank and was affected as before. All the appearances were most striking.

9073. When instead of a large flame a small round flame was placed between the Mag. poles, not above $\frac{1}{2}$ or $\frac{1}{3}$ of an inch high; the magnetic force instantly flattened it into an equatorial disc; and certainly the outer edge of this disc was the brightest and apparently the hottest, as if really the hot parts were more diamagnetic than the parts less hot or than the cooler parts.

9074. There was an effect when one pole only was used, but by no means so sensible in proportion as with two poles.

9075. Even a *glowing taper* spark produced most beautiful effects. A piece of green taper goes on glowing and sending off a thin stream of smoke. When such a taper is held under the axial line, so that the line of smoke ascends on one side of the axial line, the magnetism deflects the smoke outwards considerably. When it is held so that the smoke rises between the poles by the axial line, on making the contact the smoke line is driven right or left of the axial line, or else forks out, dividing into two lines, and the spark on the top of the wick brightens up as by a current of air blown on to it. The effect is very striking and more than expected (9088, 9154, 9155).



9076. In fact, a current of air is determined on to the ignited part of the wick in a direction from the axial line to it, and at the same time that it invigorates the wick, drives the smoke right and left. The current does not seem to originate at the axial line, but to be determined between the hot air and gas at the wick and the

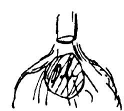
particles of air (cooler) which are next it *towards* the axial line. These relations of the hot and cold air there determine a current outwards or equatorially. I do not think much effect is produced in the rising and cooler parts of the smoke, but that they chiefly serve to indicate the currents finally established in the air.

9077. I introduced the wire of a gold leaf electrometer into the flame when afftd. by magnetism, but could not perceive the least signs of free electricity. The effect appears to be purely diamagnetic.

9078. By using little bits of paper dipped in Mur. acid and Ammonia solution, could produce visible indicating streams of fume in the cold air: but these ascended and descended between the poles, being in no way affected by the magnetism, and I became satisfied (thoroughly) that when there was no flame or spark or difference of temperature, then there was no tendency to currents or motion in the air.

9079. But as hot and cold air differ, hot air seeming to be diamagnetic, so different gases may differ though at same temperature.

9080. Made some fuming carbonic acid (by putting carbonate of ammonia into strong sulphuric acid mixed with a little nitric acid) and then poured it out of a quilled receiver, so that it formed a visible stream about the size of a quill descending between the poles of the magnet. The moment the magnet was effective, the carbonic acid was affected and opened out into a fan shape in the equatorial direction.



9081. Hence Carbonic acid at common temperatures in air is affected, is diamagnetic to the air—which therefore should be less so or be magnetic (9145).

9082. Hence flame is only a particular instance of a general case. But if heat makes air and elastic media more diamagnetic than when cold, then it is an important special case.

9083. If the heat only acts by ex[p]anding the air, it is not an especial case, but I incline at present to think that it acts specially, because we should expect that expanded air would be less diamagnetic than denser air—unless indeed air be MAGNETIC and not diamagnetic, which is hardly possible.

9084. Flame of a taper contains Carbonic acid and so far the flame would be affected as Carbonic acid. But it is affected more

than carbonic acid and therefore there is some peculiar additional effect in the same direction.

Must try other gases, heavy and light.

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9085. Tried various flame[s] between the poles of our great Electro Magnet.

Flame of	Alcohol	affected as the taper flame.		
,,	Ether	Do.	,,	,,
,,	Gas (coal)	Do.	,,	,,
,,	Phosphorous	Do.	,,	,,
,,	Sulphur	Do.	,,	,,
,,	Hydrogen	Do.	,,	,,
,,	Camphor	Do.	,,	,,

9086. So all these flame[s] were affected in the same way—but not apparently with equal strength. Thus Phosphorous flame appeared more strongly affected than coal gas flame. Sulphur flame also less than phosphorous. Hydrogen flame not so much as was expected.

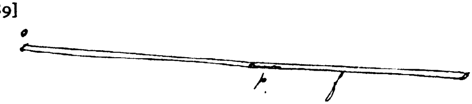
9087. There are several causes which affect the diamagnetic condition of the flame, and these should be considered in observing the result. The more rapid the ascending matter moves, the less is it deflected from the vertical line. On the other hand, the hotter the flame, i.e. the more heated the gaseous matter, the more should it be deflected. Solid particles are more diamagnetic than gas and so the smoke and flame from phosphorous more sensibly affected.

9088. A spark at end of a wood match—or a piece of amadou burning, does as well as a glowing green taper (9075).

9089*. Wishing to use Phosphorous smoke as a substance to make air and gases visible, I first tried its power alone. Taking a piece of quill tube about 2 feet long and putting a piece of phosphorous in the middle (*p*)—then lighting it by a lamp applied outside and also heating the tube lower down at *f*, the air could be made to pass up and out at *o*, visible by the smoke of the phosphorous and yet reduced nearly to common temperature.

9090. The smoke made a good indication of the presence of the cloud or air, but then, it was *clearly diamagnetic* to a certain degree. This was the more evident because the little cloud was so nearly

* [9089]



the weight of the air as to have very little ascensive or descending power in it, and was free to manifest the currents formed by the diamagnetic conditions of it and the air. But still, it must be remembered when used to make gases visible.

9091. It also shews how beautifully the dimagnetic condition of solid or liquid particles is rendered evident, when they are floated in a quiet atmosphere.

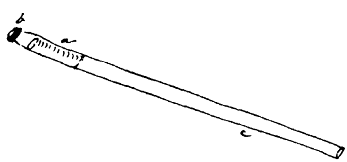
9092. To produce a stream of hot phosphorous smoke, a piece of phosphorous placed in a little cup made in chalk was burnt, so that the line of smoke should ascend between the mag. poles or on one side or other of the axial line. The diamagnetic condition of the hot stream was very evident, but not so evident as in the case of the cold smoke, and I believe only because here the ascensive power of the hot column overcame the deflexion which else would have occurred with an equally dense cold column of particles. On raising and lowering the phosphorous, so as to have hotter or colder parts of the stream, the upper cooler part shew more effect than the lower hotter part; though as to air at least, the hotter it be the more diamagnetic it is.

9093. With plates of glass and mica, I have built up a *chamber* round the Magnetic poles, which keeps the air perfectly quiet there: it can be opened at the top or sides or bottom, if needful, for introducing the currents of smoke, etc. and also for letting out fumes of gases, etc.

9094*. By having a tube of this form, about 15 inches long and $\frac{1}{3}$ of an inch wide, and contracted to $\frac{1}{5}$ or less at *b*: by placing a piece of bibulous paper inside at *a*, moistened with a strong solution of ammonia; and a little ring of tow or string at *b* moistened with muriatic acid (fuming); and by applying the hand or a lamp flame at *c*, a visible current was obtained at *b*. For the warmth made the air ascend, and it, taking up a little ammonia at *a*, when it came to the outside, found muriatic acid vapour at *b*, and so made a smoke of muriate of ammonia (9109).

9095. When in the position figured, the ascending fume was sensibly affected. So that the particles are diamagnetic. Whether the effect is due to the solid particles—or the little warmth of air, or both, the effect shew[s] how delicate this method of floating the bodies to be affected, in the air, is.

* [9094]



9096*. *Hot air.* Wished now to compare hot and cold air, differing in nothing else but temperature. Arranged a helix of fine platina wire between the magnetic poles and under the axial line. Made it red hot by a voltaic battery. The hot air from it ascended up between the poles, just as the flame of a candle had done before (9071), and a finger applied above the axial line was heated and could not be retained there. On throwing the magnet into action, this current was immediately destroyed—the finger felt cool air passing under it—but on carrying the finger right or left, i.e. across the axial line, there the heat was felt again, rising in two streams just as the two parts of the divided candle had been seen to rise before. On breaking the magnetic condition, the heated column again rose upright—and these effects could be repeated continually.

9097. So effect of heat *alone* is beautifully clear and distinct in air—hot air is more diamagnetic than cold air.

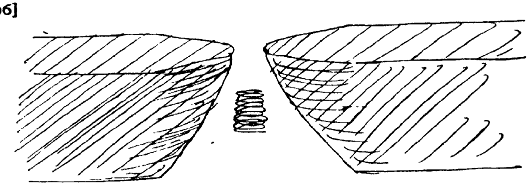
9098. When the magnetism was on, the platina helix became twisted oblique and gave a chattering sound. The twist was *one* way if the current was in one direction through the helix and the other way if the current was in the other direction. This strong appearance was simply due to the tendency of the currents in the opposite sides of the helix to *move across* the strong magnetic curves. My old effect.

9099. At the same time, there was a continual chattering noise—produced in this way: the inclining of the helix coils brought them in contact; that cut off the current from the part included, and then the coil opened; to the renewal of the current and reclosure of the helix by inclination. This stopping and making of the current in the parts of the helix produced the sound before noted in the small wire (9048), which is also De la Rive's effect.

9100. *One magnetic pole*, either knobbed or a cone, does not produce much sensible effect. Flame, however, does go out from it, being repelled. A glowing taper opposite to it, even when the apex of the cone, is but little affected. A plate of glass or mica held at the outer side of the flame or spark, so as to give equatorial direction to any current of air that might be formed, did not add any thing to the effect.

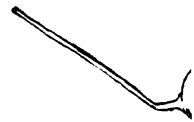
9101. A mixture of chlorate of potassa and sulphuret of antimony,

* [9096]



with so much sulphuret as to burn with slowness, i.e. much slower than gunpowder—makes when fired by a hot wire a good dense smoke that holds its state for some time—and it may be burnt in a flask or globe without danger of bursting the glass vessel.

9102. A quilled receiver had the quill made of a piece of thin glass tube about $\frac{2}{5}$ of an inch in diameter, bent as in the figure. A little of the chlorate mixture in a copper spoon was fired in the globe by a hot wire and thus the globe filled with smoky air. After a few minutes to allow of cooling, the globe was held with the quill downwards, and in the course of a minute, a very gently visible smoky stream issued downwds. from the end of the quill. This stream, brought to the sheltered magnetic field and examined, was found to present traces of diamagnetic action. It could be deflected a little right and left, but could not be broken into a fork.



9103. The same Globe, when cleared out, had a tube of condensed *sulphurous acid* introduced into it and opened; and then a little more of the chlorate mixture was fired in it, so as to fill the globe with a cloudy sulphurous acid. When the Globe was inclined as before, this gas easily poured out from the end of the quill in a *quick stream*, visible. Still, notwithstanding its quick descent, it was readily affected by the magnetic force as *a diamagnetic body*; for when the stream was passing right or left of the axial line, it was deflected outwards, and when it passed across the axial line before the magnetism was on, the addition of its force made the stream fork beautifully, so that *all* passed in two streams to the right and left. On taking off the magnetism, the two stream[s] contracted instantly into one, going between the poles—and opened out into two on restoring the magnetism.

9104. I believe therefore that Sulphurous acid is distinctly and clearly *diamagnetic* in air.

9105. *Muriatic acid.*

This gas evolved from Sulphuric acid and Mur. Amm. in a retort with a clean long small neck, and the stream of gas delivered between the poles of the magnet. It was very doubtful if it were affected when alone. If a piece of paper or little ball of tow dipped in ammonia were brought near the stream, so as to make more visible fumes, these shewed diamagnetic force; but then the

particles themselves of this smoke are diamagnetic (9095, 9102, 9109).

9106. *Silicated fluoric acid* gas. Made in a retort and tested in the same way. The results doubtful.

9107. *Fluo boric acid.* Made in same manner generally—but the preparation bad and effect doubtful.

9108. As sulphurous acid goes equatorially in air, so air ought to go axially in sulphurous acid, and hydrogen ought to go axially in air. If so, then the cases of one solution in another in my paper have their parallel in air and gases, and air may be made to seem either Magnetic or diamagnetic at pleasure.

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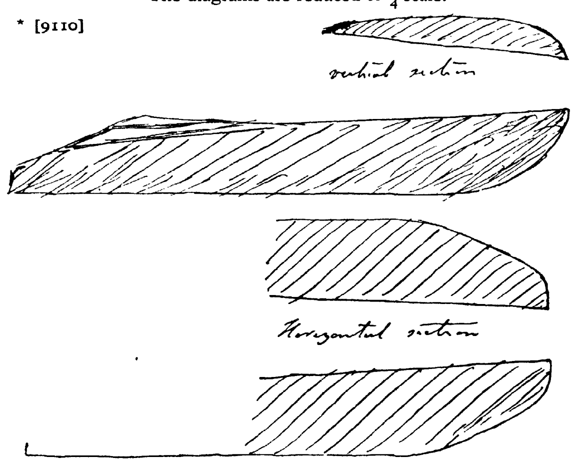
9109. The indicating tube (9094) was cloathed with paper and moistened with ether, so that when held mouth downds., a cool descending current of air fell through it, which was made visible as before by the Amm. and M.A. fumes. Still, though a cooler current and one falling in the air, it was feebly affected diamagnetically as before (9095); and therefore it is the smoke that is influenced chiefly and not the air, for the air was in one case warm, and in the other cool (a few degrees) and yet the motion of the smoke was in both cases the same. Hence shews power of a cloud (as liquid or solid particles) to be affected, and that it is not a perfectly indifferent indicator.

9110. I have two iron Mag. poles pierced horizontally and their sections and sizes[1] are as in margin*. Using these, I expected to

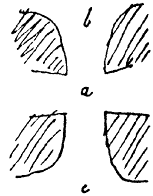
[1] The diagrams are reduced to $\frac{3}{4}$ scale.

* [9110]

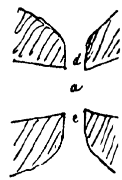


see a current set in on the flame, from the two holes in the Mag. poles, these being in the axial direction.

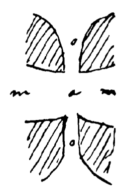
9111. But when the poles were in place and a taper flame held between them, it was but little affected at first, and after a moment or two was affected but took up an *axial form*: for it stretched out horizontally in the axial direction and the ends of the flames went each way into the apertures nearly an inch.



9112. Found that this was due to the largeness and flatness of the faces of the poles and their vicinity to one another. When the poles were as in the margin (in *plan*) and the flame was at *a*, it appeared to be but little affected; but if it were at *b* or *c*, then it was deflected outwards or equatorially. This was because of the relative flatness of the two faces as compared with the former poles (9096, 9117), so that the magnetic curves or lines about *a* were nearly of equal force for some distance towards *b* and *c*, and therefore the flame did not move.



9113. But when the poles were brought nearer to each other, then the flame at *a* expanded axially and entered both holes, not because it was then magnetic, but because from the vicinity of the faces the magnetic lines of force at *d* and *e* were stronger than those at *a*; and so the flame, being diamagnetic, was urged from *d* and *e* towards *a*, and into the hollow cores, by the inset of the currents of cold and hot air, set up through their respective diamagnetic action.

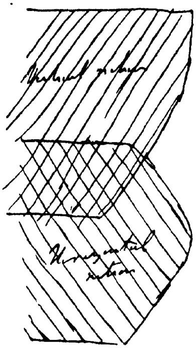


9114. So after all my contrivances, the simplest plan to test gases and air, etc. will be perhaps to put these faces still closer and then, delivering a jet of gas at *a*, see whether, being diamagnetic, it will go out by the passages in the pole terminations *m*; or being magnetic, will go out all round the poles, as at *o*, *o*.

9115. Though then *seeming* to pass equatorially, it will really be passing from weak to stronger places of magnetic action, which is the characteristic of ferruginous magnetism. It will probably pass quite away, because the air carried in with it will dilute it in comparison with [the gas] still continuing to be delivered at *a*. *Probably good.*

9116. Now used a larger flame for more visible results, as for a lecture room. A small ball of tow, about the size of a small marble, being dipped in ether, gave a flame which, held under

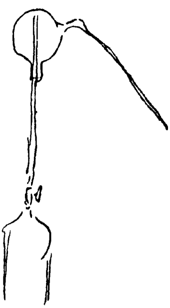
the pierced poles, rose up between them and ascended 4 or 5 inches; but putting on the magnetism, it gradually fell, then disappeared above the mag. field, but opened out right and left in a good large fork and *also* gave off *two other* branches, which entered the horizontal holes of the magnetic cores, and actually appeared occasionally at the outside. Breaking electric contact, the flames rose into one perpendicular flame—making it, they divided again as before. The effect is excellent.



9117. Using the smaller Magnetic poles (8675, 8975) (of w[h]ich full sized sections[1] are in the margin)—the flame of the same ball and ether gave an excellent equatorial division when magnetism was on.

9118. A tow ball as large as the end of the thumb gave a still greater flame, which divided excellently well. It is well to apply the tow ball *close up* beneath the poles, and not to hold it at a distance lower down, for then the current at the part between the poles due to the heat is stronger than that which the diamagnetic power can produce, and the flame, though affected, is not divided.

Now worked with gases.

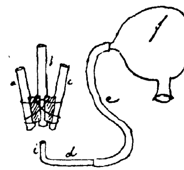


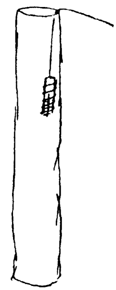
9119. *Hydrogen.* The receiver (9102) had smoke formed in it (9102) and then hydrogen was thrown out of a transfer jar by a rising tube into the globe, until one half the smoky air was displaced, after which the contents of the globe were mixed and gave a smoky visible atmosphere, consisting of half hydrogen, half air. This could be easily poured (inverted) from the end of the tube and gave a very good ascending visible column of the mixed gases.

9120. When the stream was delivered *under* the axial line, and rose by it between the poles, the magnetic action was found to affect it. When the stream was a little to the right or left of the axial line, it was very *clearly repelled*, being diamagnetic, the pointed poles (9117) being here used. There was no mistake about it. The only question was: do the visible smoky particles confer this diamagnetic character? For I was expecting to find hydrogen, and therefore the air and hydrogen, would arrange itself, under magnetic force, axially.

[1] Reduced to $\frac{3}{4}$ scale.

9121. Now arranged an apparatus thus. Three thin glass tubes *a*, *b*, *c*, were tied together by thread and blocks of cork, and placed just over the equatorial line and resting on the edges of the magnetic poles*; another tube *d* was fixed below, under the magnetic axis and about $\frac{2}{3}$ of an inch below the end of *b*, to deliver a vertical stream of any required gas; a flexible (vulcan rubber) tube was slipped on to the end of this tube and also on to the short quill of a globular receiver *f*. A coil of bibulous paper was put into the part *i* of the tube *d*, and moistened with strong solution of muriatic acid; it thus formed a lining to the delivering tube. Folded pieces of bibulous paper (about this size) held by copper wire were dipped in strong solution of ammonia and put into the upper part of each of the three tubes *a*, *b*, *c*. In this case, if the globe was filled with any gas lighter than air, the gas remained in the globe when in the position figured, but when the globe was depressed below the level of the tube *d*, the gas would go out there between the two poles and ascend up the central tube. The muriatic acid that it would take up at *i*, uniting to the ammonia it would find in the tube above, would shew by the formation of mur. amm. cloud, whether and which tube it passed up; and yet whilst passing the magnetic field, the gas would be clear and free from visible fume, so contain no solid or liquid particles to confer any diamagnetic action and thus disturb the result.

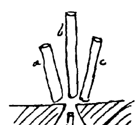




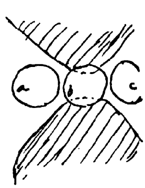
9122. *Hydrogen* unmixed with air was introduced into the globe and experimented with. The effects were very clear and plain, yet very unexpected, for the hydrogen proved to be well diamagnetic, though so light a gas. When there was no magnetic force, a fine stream of smoke rose from the top of the central tube *b*, but on making the magnet active, that stream quickly diminished, stopped and at the same time two other streams appeared, at *a* and *c*; breaking the magneto electric contact, these ceased and the upward current again appeared at *b*, and so on at pleasure.

9123. The cage of mica was at the same time roughly built up about the magnetic field, to prevent disturbance of the air (9093).

9124. When the tubes were placed in the axial direction, the hydrogen rose well also, so that without the magnetic force there was smoke at the top of *b* but none at *a* and *c*. When the magnet was on, the smoke at *b* diminished and ceased, but none appeared



* [9121]



at *a* and *c*. Shewing that the hydrogen had not gone axially but equatorially.

9125. All the experiment proved that Hydrogen was to cold air as flame or hot air to it, and divided or passed equatorially or diamagnetically (9262).

9126. *Coal gas*. Employed just the same apparatus (9121), using coal gas in the globe receiver; this is lighter than air, as is well known, and gave the central up current strong. But when the magnetism was on, the gas was powerfully affected, for the up center current ceased and the two side equatorial current[s] were well produced (9122). This gas is more diamagnetic, I think, than hydrogen. At all events, the effects are more visible and striking; but that *may* depend on the circumstance that hydrogen by its lightness forms a powerful up current and so in some degree mechanically diminishes the equatorial effect.

9127. With larger tubes and more Ammonia, will be a good expt. for the lecture room.

9128. Appears therefore that it is not merely heavy gases that go equatorially or are diamagnetic, for light gases as hydrogen and coal gas do not go axially, but are the same as Carbonic acid and sulphurous acid, i.e. are diamagnetic in air.

9129. Air therefore ought to go axially in Carbonic acid gas or in any of these gases.

9130. *Nitrogen* was put into the Globe (9121) and an ascensive power given to it by making the globe and the tube warm. There were perhaps some signs of effect, but they were so small that I could not make them out certainly, or tell in which direction the effect was. The signs were a momentary shake of the smoke at the top of the central tube on making or breaking contact, but on the whole I think *Nitrogen is nearly as air* (10865).

Perhaps the new arrangement may tell this better (9114).

9131. *Oxygen*. Instead of the flexible tube *e* (9121), had a glass tube two feet long descending nearly vertically and dipping into a bottle of oxygen, then made this tube hot by a spirit lamp and so caused an ascending current. But I could perceive no magnetic or diamagnetic effect.

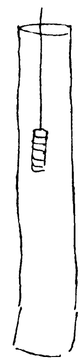
9132. Mixed 4 vols. of oxygen and 1 vol. of hydrogen, so as to have a rich oxy. mixture with ascensive power. This mixture

delivered from the globe (9121) rose well, and was slightly *diamagnetic*; but I think that that was due to the hydrogen present and that the oxygen is scarcely different from air.

9133. These experiments would shew that oxygen and Nitrogen are not much different from each other, though both differ very much from the other heavier and lighter gases tried. This is, in respect of the constitution of air, a very remarkable relation between them (10866).

9134. *Ammonia.*

The indicating ammonia was cleared out of the three upper tubes, *a*, *b*, *c* (9124). The lower delivery tube *d* was replaced by another with no muriatic acid in it. A retort was charged with a mixture of Mur. of Amm. and quick lime, and being connected by a flexible tube with delivery tube *d*, the mixture was heated. The ammoniacal gas set free passed well between the mag. poles and up the central tube *b*, so that if a folded piece of paper dipped in Muriatic acid were held in the tubes *a* and *c*, no fumes appeared there; but holding it in tube *b*, dense fumes were formed which were carried quickly up and out of the tube. But on rendering the magnet active, the current of these fumes stopped, and then became descending; at the same time, ascending currents of ammonia were found in the equatorial tubes *a* and *c*. On breaking the magnet connexion, these instantly changed, ceasing in *a* and *c* and being renewed in an upward direction in *b*. Ammonia is well diamagnetic though lighter than air, and it seems to shew the effect of the *large proportion* of *hydrogen* in it.



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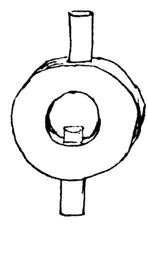
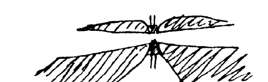
9135. Have made a cork chamber to place between the two magnetic poles, with a tube in below and out above, so that when put close in between the pierced poles (9110), it with them formed a flat hollow chamber with four apertures, two vertical and equatorial as the tubes, and two horizontal and axial as the passages in the magnetic poles. So when vapour sent in below or above, it was free to go out *equatorially* above or below, or else *axially* at the passages in the poles*.



9136. Held a smoking taper below so that the smoke ascended into the chamber; then on bringing on the magnetism, there was

* [9135]



suddenly a push of the smoke in the chamber into the spaces in the sides of the poles, but the axial flow of smoke did not continue.

9137. With one of the pole passages stopped, the same thing occurred.

9138. Indeed, this ought to be so, as I see now, for the actions are the result of the different matters found in the vicinity of the pole—so whilst both smoke and air in the chamber, the air would draw the smoke out, but if all so well mixed up together as to be alike, then no motion; so the magnetism caused motion until the differences were levelled and then there was no more. That is, to have a continuous repulsion, we must arrange circumstances so as to give a continuous difference in the Mag. field (a body in the axial line that is repelled, and a body round it that is attracted)—speaking of the ordinary and solid form of poles.

9139. Instead of smoke, sent in *coal gas* from below. There was an occasional, but not a good effect—not so distinct and clear as with the three tubes—for *air* is required simultaneously round the Magnetic field as well as coal gas.

9140. After a good deal of trial, found that coal gas passed *axially* very well through the pierced poles (9110) when the poles were within 0·3 or 0·2 of an inch of each other (being stopped open by a piece of copper, that they might not fly together); the cork chamber being away and the sides quite open; a glass tube hung over the top with ammonia paper in it—and one of the pierced pole passages corked up. Then with M.Acid in the supply tube to serve as indicator, with the ammn. above, and the coal gas delivered just below the edge of the pole holes, it went *well upwards* when there was no magnetism and *well axially* through the magnetic poles when magnetism on.

9141. The little M.A. vapour used is soon absorbed by vulcan tube or else by the coal gas or by both.

9142. So pierced pole form will do, but not better than the three tubes (9124, 9200).

9143. Now arranged the solid poles with the three tubes above (9124), to which dropping paper sides and ends had been added—raising the tubes so that air could enter between the lower edge of the central tube and the poles (which is necessary). The jet

supplying *coal gas* was above 0·7 of an inch below the top surface of the poles and the poles about 0·2 of inch apart. This gave an excellent effect, testing by M. Acid and Amm. as before (9121).

9144. The three tubes are each about the size of my little finger and the ammonia sol. paper is in each about half way down, the tubes being 3½ or 4½ inches long.

9145. *Carbonic acid.* The three tubes were now placed below, but the poles were not turned half way round but kept with flat ends upwards. Carbonic acid rendered visible by nitric fumes (9080) was poured as a jet as on the former occasion and its course observed. It was manifest that the current should not be delivered by an application of the tube mouth two [too] close to the poles, for then it has not time, and is not at liberty to be deflected in its approach to the poles or axial line, and the atmosphere at the poles is too much carbonic acid. When the end was one inch off, and the stream of gas going clear through the air of the size of a small quill, it was in best condition, and then it was manifest that, air being in the magnetic axis, it tended to keep there, for the C. Acid was thrown away right and left. The air at the axis is then probably stationary or tends to be, and being held there, acts as a barrier to the passage of the C.A.; and between it and the C.A., other air is urged in by the relative forces of attraction or repulsion, i.e. the diamagnetic forces. When by putting the tube close the magnetic axis was flooded by a continual stream of Carb. Acid, it was not so easy to see the effect of the air to displace it, for fresh carb. acid falling by its gravity always replaced the receeding portion.

9146. By watching the form of the stream of C.A., it was easy to adjust it, so that it fell well either through the central or the side tubes, as the magnetism was off or on (9263).

9147. The present disposition of the poles with flat face upwards seemed quite as good as the contrary position, i.e. if one can see where the gas is and is going to.

9148. My present impression is that for a good result, we must not *drown* the magnetic field with gas. Nor have *too large a stream of gas*, but rather imitate the smoke of the glowing taper. Also that the poles may be *too near* each other.

9149. Tried *cold air*, letting a stream of it fall from end of a

metal tube cooled by freezing mixture inside, and tested for the place of descent by a glass tube below, having a compound bar of silver and platina formed into a spiral within. Let the current fall at one side of axial line and received it by the testing tube below —then on making magnet, the spiral indicated a little increase of temperature, as if the cold stream which was before falling through it, was now drawn *to* the axis, being magnetic relatively to air at common temperatures—but the effect was small and I must have a better apparatus and clearer results.

9150. May observe that as a fact, it has not yet been shewn that cold air is not at Zero between diamagnetic and magnetic bodies. Still, it is not likely it should be there, because hot air is diamagnetic (9096, 9097), and hot and cold probably only differ in degree and not in kind—but as yet we don't know.

9151. Air, or oxygen and nitrogen, is of all gases yet tried the least diamagnetic—and it *is possible* that one of these gases may at a certain temperature be at Zero (like a vacuum), and if hotter, diamagnetic; if colder, Magnetic like iron. But *probably* is not so (10866).

9152. *Nitrous gas.* Sent a stream of Nitrous gas upwards from below, testing by ammonia in the three tubes above. The stream was far too large and unmanageable, and the gas in the air became Nitrous acid gas, and also produced heat. On the whole, the result of the action was that the products were affected and were *diamagnetic*, but the change of substance—properties, density, temperature, etc. give so many conditions, that it is only the general result that can be stated. The effect cannot be referred to one cause or body only.

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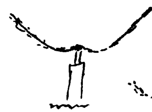
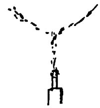
9153. Have had some separate conduction tubes mounted to catch the gass after it has passed the poles, and use either one, two or three at pleasure. Are very good and convenient in adjustment, and are about the size and length of the middle finger.

9154. Put up the solid poles, and by a glowing green taper examined where best the issue of gas should be to give best divergence. When the poles were 0·25 of inch apart they were good: and when the glowing end of taper was half an inch below





the level of the top of the poles, the effect was so great that the smoke was actually driven down and a current of fresh air descended to the wick. If the taper ignited and held lower down (as an inch below the top level), the smoke does not divide and fork so soon, for it rises until about $\frac{1}{3}$ of an inch below top level and then divides, but not so forcibly.

9155. In the latter case the smoke is not so hot as in the former, and so here again the dependance on heat shewn.

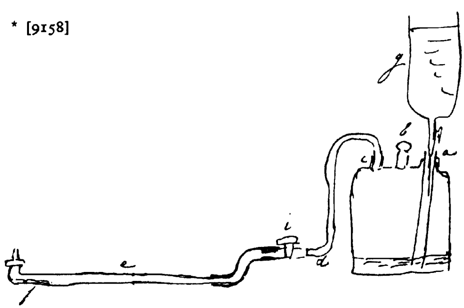
9156. However, at the distance of half an inch below top level, there is excellent dividing power, and the separate tubes applied over the equatorial line act admirably. The smoke easily goes up the middle or up the side tube at pleasure, and with a large smoking taper and long tube the experiment is an excellent one for an audience.

9157. In investigating experiments, may either use no middle tube or one higher up—a single side tube is perhaps best and the up stream a little on one side of the axial line. It is certainly best for taper smoke.

9158*. Have arranged an apparatus for a stream of gas. It is a Woulf's bottle with three apertures. Aperture *a* has a wide tube fixed into it, which goes to the bottom and is open there. Aperture *b* is stopped—aperture *c* has a bent glass tube reaching to *d*—there a stop cock is attached to it and beyond that a vulcanized flexible piece of tube, to join on at pleasure to the exit tube *e*; this has a little paper at *f* moistened in Sol. Mur. acid, and the end is turned up and cupped—a little glass mouth piece fixed in a bit of cork. This can be adjusted so that the end shall deliver a vertical stream of gas under the axial line or in any other part near it.

9159. Then *g* is a jar of water with a stop cock partly plugged by a wooden rod, and this enters the tube at *a*, so that when the cock is open, the little run of water enters the bottle, and the cock *i* also being open, drives an equal portion of gas through the tubes and out at the jet in the magnetic field.

* [9158]



9160. Coal gas was then introduced into the Woulf's bottle and afterwards delivered out below the axial line by a jet of water delivering only 12 cubic inches in one minute. A side tube with ammonia paper (9153, 9134) in it was applied, and the stream was delivered a little on one side of the axial line. When not magnetic, the stream ascended vertically and missed the side tube: when the magnet was active, it deviated and entered the tube and rendered the M.Am. smoke well visible above. The effects were very good and this arrangement will do well.

9161. *Olefiant gas.* Think it is diamagnetic, but the result is doubtful, for having little ascensive power in air, if any, it did not appear in the side tube, but seemed driven down and away altogether. Must try it in a descending current—or cold—or else in Carb. acid (9172).

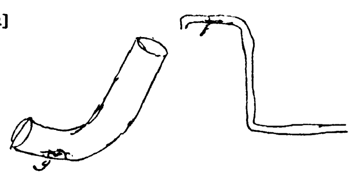
9162. *Nitrogen.* When not magnetic, ascended very fairly by a middle tube—and it was certainly affected and diamagnetic when the magnet was on. The effect was evident mainly in that when the magnet was made, the ascending current was interrupted and diminution of the smoke at the top of the middle tube occurred, and also more at the side tube; these returning to the former state on making the magnet inactive. As this gas is light enough to ascend in air and powerful enough to shew this difference, it ought to be considerably more diamagnetic than oxygen since $\frac{4}{5}$ of air is nitrogen (10865).

9163. Also, Oxygen ought to appear *magnetic* in air (9168).

9164. Proceeded to experiment with gases heavier than air—and for this purpose the conduit tube *e* (9158) rose up so as to deliver the gas downwards between the magnetic poles, and the catch tube was bent into this form* and placed below on one side. Mur. acid sol. at *f* and Ammonia solution at *g*.

9165. *Carbonic acid* was first employed and acted beautifully. It was delivered a little on the tube side of the axial line, but fell downwards and quite clear of the tube *g*, whilst there was no magnetic action. But when the magnet was active, the stream of gas was deflected outwards or equatorially, and entering the catch tube *f*, flowed down it and sank over its lower edge in a visible stream. The effect was beautiful, only as the gas is heavy, the smoke must be looked for downwards. The

* [9164]



effect is easily intermitted or repeated, and always excellent (9263).

9166. I cleared the ammonia away from the lower or catch tube and then placed a glass with lime water under the edge of the tube. There was no signs of any carbonic acid there whilst there was no magnetism, but on bringing on the magnetic force, immediately the carbonic acid descended into the glass and made its self visible in the turbidness of the lime water, which was kept in a stirred condition during the experiment. Beautiful effect.

9167. The supply of gas is quite enough at the given rate of (9160), 12 cubic inches per minute.

9168. *Oxygen.*

When the stream of oxygen was delivered downwards over the axial line and between the poles—the rendering the magnet active caused no change or deflection of any kind: there was *no* equatorial or diamagnetic action. When the stream was delivered on one side of the axial line and over the side ammonia tube (9153, 9160), without the magnetism it passed through the tube, making its course visible by the smoke finally produced there; but on making the magnet active, the oxygen stream left the tube and approached the magnetic axis, being *attracted* and therefore in air to all appearance being magnetic (9163). Stopping the magnetism, the stream returned to the perpendicular and entered the tube, to be again attracted and deflected on renewing the magnetism.

9169. So Oxygen magnetic in relation to air and much aside or apart from Nitrogen. Also, if a diamagnetic, the least so of all bodies yet tried amongst gases (10866).

9170. *Carbonic oxide*, freed from Carbonic acid by caustic potassa. This gas was very diamagnetic, so that the gas was driven up and over the side descending exit tube. By putting a little ammonia in the mica chamber (9093), could see this very well. The moment the magnet was active, the descending stream of gas started upwards and sidewards equatorially out of the magnetic field. It is apparently more diamagnetic than Carbonic acid, but must remember that it is much lighter and descends in the air mainly by the momentum it has at the mouth of the delivery tube; so being lighter, it is more easily driven upwards, for gravity does not counteract the dispersion.

9171. A little ammonia in the Magnetic chamber helps well in these cases to distinguish the effect, Mur. acid being in the gas delivery tube (9164).

9172. *Olefiant gas* being tried as a descending current (9161) was found to be well diamagnetic. Seems the more so because nearly of the same S. G. as the air (10867).

9173. Air with *Ether* vapour (saturated)—doubtful in effect, not easily rendered visible. If necessary try hereafter by [illegible] the atmosphere and [illegible] it.

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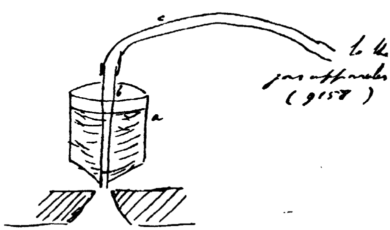
9174. *Nitrous oxide*, being tried as a descending current (9164) was moderately but clearly diamagnetic. Now this gas contains more oxygen than air, yet diamagnetic in air. On the other hand, it is denser than air and therefore contains more matter; it also contains more nitrogen than air, for 1 vol. contains 1 vol., whereas of air 1 vol. contains only $\frac{4}{5}$ of a volume. Still, the result is, I think, in favour of oxygen being a diamagnetic and not a magnetic (10868).

9175. *Nitrous gas*. Made it a down current at first, but could distinguish nothing because heat of combination with oxygen of the air sent the products up. Then made it an up current. Cannot make out the products diamagnetic, but rather magnetic or axial in air. Consider that Nitrous gas and nitrous acid are both present, and also heat, adding its influence, and that magnetic field is swamped in these things, and its common air changing at the same moment (9264).

9176. Still in its uncertain or magnetic state see the effect of much oxygen.

9177*. *Cold*. Proceeded to experiment upon the effect of cold in rendering air magnetic in common air, etc. *a* is a cylinder of zinc, about $1\frac{1}{2}$ inches or 2 inches in diameter, and $3\frac{1}{2}$ or 4 long, having a copper pipe *b* passing through it, about 0·2 of an inch in diameter at the lower end; *c* is a vulcanized tube going away to the gas apparatus. The vessel *a* was covered externally with flannel to the edge of the lower aperture of *b*, and being filled

* [9177]



with a mixture of salt and ice pounded together, was fixed up over the magnetic field, so that any air or gas sent in by the gas bottle and water (9158) should be cooled before it passed as a stream between the magnetic poles.

9178*. For an indicator, an air thermometer of about this size[1], having a few divided portions of an alcoholic tincture of cochineal in the stem, was fixed by a cork into a catch tube (9153), and then placed so that the mouth of the catch tube was under the equatorial line on one side of the axial line. Then by shifting the refrigerating apparatus, it was easy to let the cold stream of air either fall between the poles, or into the catch tube, or between the two, i.e., on one side of the axial line. When the cold stream entered the catch tube, it was instantly made sensible by the effect.

9179. *Air cold, at* 0°, in air at 60°. There was a clear effect though small; the cold air went axially or was less diamagnetic than air at common temperatures. The effect was difficult to catch, and for some time I thought it doubtful.

9180. *Oxygen cold*, in air at common temperature, 60°. The cold oxygen was drawn towards the magnetic axis very instantly—but as oxygen at common temperature would do the same, the effect only shews that it keeps that relation at cold temperature as 0° or 10°. The experiment is another proof that oxygen is magnetic in air.

9181. *Nitrous oxide cold*, in air at common temperature, 60°. Very little influence of the magnetism wherever the catch tube or the descending current was placed, so that the Gas and air are nearly alike. Therefore the cold has had no effect, except it may be rendering the nitrous oxide a little less diamagnetic than before (9174), and so bringing it still nearer in that respect to air, than when both are at the same temperature.

9182. *Carbonic acid gas cold* in air at common temperature, 60°. The carbonic acid seems scarcely affected at first; but made a fresh cooling mixture and stopped up bottom of the catch tube (9153, 9178), and now found that the cold gas diamagnetic in air, but I think much less so than at common temperatures.

9183. *Olefiant gas cold* in air at common temperature. Was well diamagnetic, far more than Carb. acid. The bath was at 10° F.

[1] Reduced to $\frac{3}{5}$ scale.

* [9178]

9184. *Nitrogen gas cold* in air at common temperature. Was sensibly diamagnetic. When the descending current was over the catch tube, the coldness was indicated by the air thermometer, but when the magnetism was put on, the temp. rose because the nitrogen was thrown up into the general mass of air. Temperat. of vessel 20° after the experiment finished.

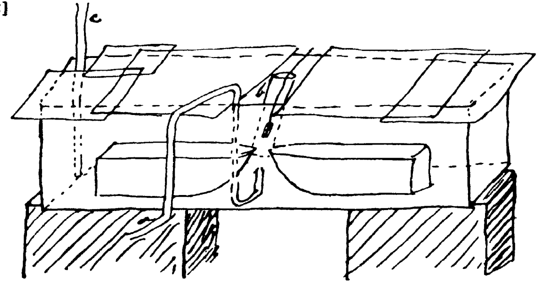
9184½. So this cold makes no violent change in any of these bodies, only a difference in degree, diminishing the diamagnetic force.

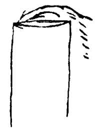
9185. Substituted an atmosphere of Carbonic acid about the magnetic poles for the common air, thus. A double vessel made of waxed cartridge and filtering paper was folded up with dog's ears corners so as to be 13 inches long, 5 inches wide and 4 inches high. This being placed on the great magnetic poles, the terminal pieces of iron were put inside of it as above*, at the proper distance. Then a bent glass tube *a* was fixed up, so that it delivered a jet of gas under the magnetic axis (or on either side of it, by adjustment), its outer end being connected with the gas supply (9158). A catch tube *b* sustained by its own stand was adjusted by the side of the magnetic axis—and a third tube *c* which came from a carbonic acid generator passed to the bottom of the paper vessel. The vessel was covered with plates of mica laid loosely on the top.

9186. I found it easy to fill this paper vessel with Carbonic acid, and to keep it full, supply more of that gas between every experiment by the tube *c*. I found it also easy to make experiments in this atmosphere with gases lighter than Carbonic acid. The following are the results.

9187. *Common air*. It passes towards the axial line, appearing to be *magnetic* in Carbonic acid, and this very distinctly. For the jet being arranged on one side of the axial line, and the catch tube over it, and M.Acid being in the gas tube (9121) and ammonia in paper in the catch tube (9121), the common air could,

* [9185]





by looking horizontally, be seen ascending up the catch tube like a little spring and flowing away downwards over the edge. It ascended because, mingling with the C. Acid, it was still lighter than it; but descended in air because of the C.A. with it. On making the magnet, this current instantly ceased, for the air stream then went toward the axis, escaped the catch tube and appeared over the axial line (9267).

9188. *Oxygen stream*, in atmosphere of Carbonic acid gas, went well towards the axial line.

9189. *Nitrogen* is *diamagnetic* in Carbonic acid, but not so strongly as in air.

9190. *Hydrogen* clearly and well diamagnetic in Carbonic acid.

9191. *Coal gas*. Diamagnetic in Carbonic acid.

9192. *Olefiant gas*. Do.; being heavier than coal gas, it does not shew so good a spring up as the former gas (9191).

9193. *Carbonic oxide*, very fairly diamagnetic in Carbonic acid gas. Here the condition of oxygen will shew, and its force. Equal volumes being taken, the quantity of carbon is alike in both, but the quantity of oxygen is twice as great in Carb. acid as in Carb. oxide. Now though the additional oxygen is actually added and compressed in the volume of C.A. (as compared to the C.Ox.) it does not add to its diamagnetic condition, but takes from it.

9194. This manifestly shews that either oxygen is really on the magnetic side of 0°, or that compounds have specific diamagnetic force which is *not* the sum of the forces of their particles.

9195. *Nitrous oxide*. Being heavier than Carbonic acid, tried it by a falling stream, and found it slightly diamagnetic.

9196. *Ammonia*, from a generating retort (9134), was very well diamagnetic in Carbonic acid—very distinctly seen as it ascended, and as current is rapid, diamagnetism must be considered strong.

9197. Carbonic acid is an atmosphere which, because of its oxygen, is not far from air in its diamagnetic place. Would have a more striking result with Coal gas or hydrogen probably. Many other gas[es] would perhaps appear magnetic in them.

9198. As an ascertained and experimental result, where is Zero, or a vacuum, and how is oxygen to it?

9199. Our best ordinary steel magnet, with the two iron cones (8379) attached to form [illegible] poles, does shew a feeble effect on the delicate film of smoke from a small spark on glowing taper.

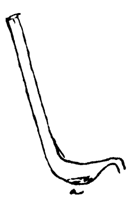
9200. Tried the pierced poles (9110) again with taper smoke, all being open around. The effect is very beautiful. One can easily by it trace out the circle of lines of maximum magnetic intensity all round the apertures of the passages, and send the smoke either inwards or outwards at pleasure.

9201. A copper cylinder, weighing about 30 grains, was suspended by a loop of platina wire, above which was a link about 6 inches long, also platina, and then about 4 feet of fine cocoon silk. By a little spirit lamp, this cylinder could be made red hot when within $\frac{1}{10}$ or even $\frac{1}{20}$ of one of the magnetic poles (9117). Comparing the effect of deflection when hot and when cold, I could not see that there was any clear difference. Perhaps it was a little more diamagnetic hot than cold, but the effect might be due to the diamagnetism of the heated air about it, even when the lamp was away. Must try bismuth (   ).

9202. A second cylinder gave the same result.

9203. A spirit lamp flame being in the magnetic field and affected by the magnet, it was searched in all directions by the two platina ends of my Galvanometer wire, but I could not find any sensible effects that were not referable to known causes.

9204. Proceeded to try certain vapours and gases in air.



9205. *Iodine*. Shaped a glass tube into the form represented, the part at *a* being about as large as the thumb. Put some iodine in and then made the lower end and especially the spout and parts near it hot by spirit lamp; then inclining it over as in the act of pouring, the iodine (melted) flowed towards the hotter parts, and a very good and visible current of purple iodine vapour descended from the little mouth of the vessel. When this was directed between the conical poles, it was soon found that the iodine was distinctly diamagnetic in air. The dense stream was so heavy as to fall almost like water across the axial line, but a stream of less colour and consequently mingled with air was well driven equatorially.

9206. *Bromine* gave me an indistinct result from feeble colour of

its vapour. Think it is diamagnetic, but must try again with the side catch tube.

9207. *Chlorine.* Used the bottle apparatus (9158) to send the gas, and had beneath the axial line a tube into a jar of slaked lime, to catch and absorb what might fall undeflected. Used the side catch tube to indicate. The *chlorine* was *well diamagnetic* in air. When magnet was not active, and an ammonia paper in the catch tube, the chlorine fell between it and the axial line; but when magnetism on, then a sudden determination of the chlorine into the catch tube, and the issue of a good cloud of smoke from it. Again, with no magnetism and a piece of litmus damp paper in the catch tube, the paper was not affected. But making the magnet active, the paper was instantly bleached.

9208. *Cyanogen.* A tube containing cyanide Mercury was attached by the vulcan tube to the delivery tube (9164), and then cyanogen evolved by heat. It was difficult to test this gas by ammonia or by sight, but I succeeded well thus. The glowing wick of a taper was held in the catch tube for a moment and thus some smoke left in it, which being soon cooled and heavy, remained there very quietly, until the magnet was rendered active, when it was suddenly thrust out below, by the rush of cyanogen and air in above—deflected by the magnet. The effect was very good and distinct. If a glowing taper were held in the catch tube below, and its degree of brightness noted, this was seen to increase upon rendering the magnet active, and as it was too far away to be affected of itself, I think it was invigorated by the current of air and cyanogen determined into the tube. Cyanogen is well diamagnetic.

9209. *Hydriodic acid.* Evolved from a retort attached to the delivery tube (9208). It was well diamagnetic. It poured beautifully (when there was abundance of gas) into the side catch tube, and when there was a smaller and lighter stream, it was actually blown up and away from both magnetic field and tube.

9210. *Muriatic acid.* Tried the same way. Well diamagnetic.

9211. *Fluo silicon.* Tried the same way. Diamagnetic in air very clearly and well. There was a strong wave each time through the catch tube.

9212. *Sulphurous acid.* A tube of liquid sulphurous acid—cooled,

opened, and then attached to the delivery tube (9208) so as to serve as generating retort. The Suls. acid strongly diamagnetic—as seen by the catch tube—but Ammonia did not shew it so well as litmus paper. This was excellent—the part outside the tube was often reddened by the Suls. acid floating about but not that inside—until on rendering the magnet active, that inside was changed instantly from blue to red. Might catch this gas into a glass with litmus paper.

9213. *Nitrous acid.* A tube of this liquid was attached as before (9208) and warmed with the hand. It gave a fair stream, and by the effect in the test tube, could see that it was, if different from air, a little axial, i.e. *magnetic* in relation to air—but not much I think.

9214*. Proceeded to try the effect of heat on certain gases to ascertain whether in all of them it increased the diamagnetic condition. For this purpose, an under exit tube (9158), in shape like the figure, was provided and fixed so as to deliver a stream (from the water bottle or other source) upwards under the axial line. Then a platinum wire helix was introduced into the mouth of this tube, and by a small battery, could be raised to a white heat or any heat under it at pleasure. The experiments then consisted in heating the helix and sending a given gas through it underneath the axial line, and ascertaining what the effect of the magnetism on it was. The apparatus answered perfectly.

9215. *Hot oxygen* is well diamagnetic to the air at ordinary temperature. It rose directly between the poles until the magnetism was on and then went right and left. The effect was very manifest either to the finger or to a spiral compound platina and silver thermoscope placed in a tube above.

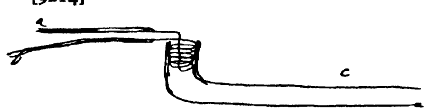
9216. *Nitrogen hot.* Strongly diamagnetic in air.

9217. *Nitrous oxide.* More diamagnetic hot than cold. Used gas at the rate of 12 c.i. per minute, and also three or four times that quantity, to observe if any difference, but effects on finger the same, or even better, with the greater quantity.

9218. *Carbonic acid.* The same, more diamagnetic being hot.

9219. *Ammonia*—hot, diamagnetic, well—beautiful effects on the finger by sensation, and also on litmus paper. With no magnetism, a piece of reddened litmus paper was blued first between

* [9214]



the poles—but if Magnetism on and then a piece of red litmus paper placed there, the bluing was first in two spots right and left of the axial line. It requires steadiness in carrying the paper to its place not to disturb the air, and also to hold it half an inch above the upper surface of the magnetic poles, that the common air may have its proper access.

9220. *Muriatic acid hot.* Very diamagnetic to finger or thermoscope or blue litmus paper. The one or the two spots indicating the places of the stream of hot Mur. acid are beautifully distinct.

9221. *Coal gas.* This gas did not inflame when the helix was made only cherry red hot, and at this heat it gave the same diamagnetic phenomena as the other gases.

9222. *Olefiant gas.* Same as coal gas and I believe more diamagnetic hot than cold.

9223. *Hydrogen gas*—could not be heated so highly, but was raised so high (the platina wire being visibly red hot) as to affect finger, etc., and was as the other gas—more diamagnetic hot than cold.

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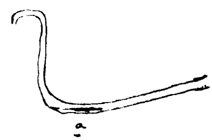
9224. A glowing taper near the axial line, and a tube about as wide as the finger and 6 or 8 inches long, held outside of it, shews beautifully the use and action of catch tubes.

9225. *Bromine* tried thus. The conveying tube (9164) which delivered downwards had some bromine put into it at *a*, and then the gas apparatus (9158) was attached to it and common air sent through. It took up so much bromine as vapour as to make a falling yellow stream, and by delivering a little ammonia near the magnetic field, it was clearly seen that this mixture of air and bromine was diamagnetic. Hence pure bromine vapour must be diamagnetic to air and far more diamagnetic than oxygen.



9226. Arranged the Carbonic acid bath, etc. (9185), and delivered the following gases from above (9265).

9227. *Muriatic acid.* It was clearly diamagnetic in the Carbonic acid.

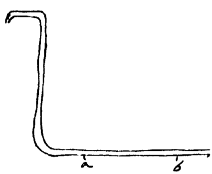
9228. *Nitric oxide*, or nitrous gas. It appeared to be slightly diamagnetic in the Carbonic acid—testing by Ammonia held over the jet of it, on one side of the axial line. But it must be remembered

that a little air may mingle with the Carbonic acid in such experiments as, like these, are open to the air, though C.A. be delivered into the vessel from time to time (9268).

9229. *Nitrous oxide* is clearly diamagnetic in the Carbonic acid. It is so nearly equal to it in weight that it is troublesome to trace its course.

9230. Tried to heat Carbonic acid in Carbonic acid by the helix and obtained a result—namely, that the hot was diamagnetic to the cold. But my helix melted and I could not repeat the experiment at this time (9269).

9231*. Arranged two magnetic terminals so as to give poles of a conical form and facing each other, but raised so high that I could cover them with a glass shade, to contain coal gas or hydrogen, and still send gases up to or down into the magnetic field by a properly shaped conducting pipe and the bottle apparatus (9158). The poles did exceedingly well and acted with a flame or a taper as before. A pewter pipe *b* was connected with an air holder filled with coal gas, and in this way it was found easy to act in an atmosphere of that fluid; but on the whole, I think I must use hydrogen, for the coal gas contains free oxygen, perhaps from air.

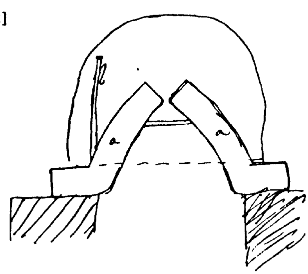


9232. In order to give the gases sent in a little muriatic acid (so as to make them visible afterwards by ammonia), I put into the delivery tube a hollow cylinder of filtering paper extending from *a* to *b*, 3 or 4 inches, and then moistened this with strong solution of Muriatic acid, so that each gas as it passed took up a portion; and as the catch tube was difficult to arrange and adjust, I used in preference a folded piece of paper at the end of a wire and dipped in strong solution of ammonia, to bring near to the stream, and so indicate its course. On the whole, this did very well—but a little allowance has to be made for the smoke effect.

9233. The following are some results—some good, others not.

9234. *Air in coal gas*, appeared to be very little affected—it was on the whole a little magnetic, i.e. went axially.

* [9231]



9235. *Oxygen* in coal gas was very magnetic and the experiment was very beautiful. The ammonia, in uniting with the M.A. of the issuing oxygen, made a cloud which fell well and quickly; but on making the magnet active, the cloud instantly ran back and accumulated about the poles, occupying the magnetic field, so that the pole ends were hid in a cloud of smoke an inch or more in diameter. When the magnet was thrown out of action, this instantly fell and was cleared away, or if allowed to fall only a little and then the magnetic force renewed, it rushed back again to the magnetic field.
9236. *Nitrogen* was clearly and well diamagnetic in this coal gas.
9237. *Carbonic oxide* was also slightly diamagnetic in the coal gas.
9238. *Carbonic acid*—was slightly but distinctly diamagnetic in the coal gas.
9239. *Olefiant gas*—a little diamagnetic in the coal gas—the gas in this case was a fresh and well prepared atmosphere of Coal Gas.
9240. *Nitrous gas.* Awkward to deal with, for it finds oxygen in the coal gas and rendered it all red. I could not determine its direction.

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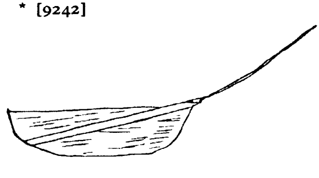
9241. Thought it possible from the effect of oxygen in coal gas (   ) that even the Ox. and Nitrogen of air might be separated more or less by magnetic attraction. If so, it would be a beautiful result—and give a new mode of comparing the relative diamagnetic condition of 2 gases mixed together.
9242*. Prepared a glass tube with a fine mouth open at both ends. This was placed in a basin of water, and the fine end just between the two magnetic poles (they being a very little way apart) so as to be in the magnetic axis. Then the magnet was made active and the water drawn off from the basin slowly by a syphon, so that the tube became partly filled with air from the Magnetic field. The fine mouth was then stopped by a little water applied with a stick, and the gas carried off and examd. It was exactly as before, common air, so that there were here no signs of separation. Hoped it might be oxygen. The experiment was well made but not long continued. Perhaps a slower action might have done something.

* [9242]



The poles were well sheltered from disturbance of the surrounding air.

9243. Bismuth heated in a little glass vessel seemed more magnetic than it ought to be, but signs were uncertain, and my suspension broke down before a fair trial.

9244. Proceeded to experiment with gases in an atmosphere of *Hydrogen*, in the apparatus before described (9231); but I have cut a mill board so that the edge of the French shade should rest on it all round, and the mill board be continuous except for the holes through which the poles and tubes passed upwards. Thus the atmosphere within the shade is kept well together and but little disturbed.

9245. The hydrogen was sent in from air holders and in the pipe was some fluid ammonia, that the atmosphere might be impregnated a little with it. Then the gases to be tried were sent in by the bottle, etc. (9158), and as before a little charged with M.Acid. So as the stream entered the hydrogen, it made itself visible enough to shew what direction it took.

9246. After the trial of one gas, then a second or another gas was tried in the same atmosphere of hydrogen. This being done, that hydrogen was replaced by fresh and the gas again tried in this hydrogen. So every gas was tried in the hydrogen of a former expt. and then in fresh hydrogen.

9247. *Air* in hydrogen. I cannot be quite sure about air in relation to hydrogen. In one experiment, it seemed to go to the axial line–in another, to be a little diamagnetic. The effect was so small that even the smoke might have produced the diamagnetic results without any action of the air (9272).

9248. As oxygen is very magnetical and nitrogen diamagnetic, it is not surprizing that the proportion in air should give a mixture nearly as hydrogen.

9249. *Nitrogen* in Hydrogen. Was clearly and strikingly diamagnetic in both the Hydrogen atmospheres.

9250. *Oxygen* in Hydrogen. Beautifully Magnetic. As the stream was delivered a little on one side the axial line, it was drawn into a circular course and revolved round the line, forming a cloud attracted to and spinning, having the axial line for its axis.

9251. When the magnetism was taken off and then put on again,

the oxygen was drawn back as before in Coal gas ( ), but not with the same apparent degree of suddenness and energy. This may be either because coal gas is more diamagnetic than Hydrogen, or because it buoys up the oxygen more, being heavier —or, as I believe, from the joint action of both these causes.

9252. *Nitrous oxide* in hydrogen. Diamagnetic—clearly. The little oxygen left in the feeding pipe from the previous experiment made a beautiful result. Whilst that was issuing forth, the stream of gas went to the axial line—but the instant the Nitrous oxide came forth, the stream was repelled and bent outwards, just as if a sudden change of wind had taken it. It was very easy to distinguish the two gases magnetically.

9253. *Nitric oxide* or Nitrous Gas in Hydrogen. This body is difficult to expt. with, because it soon renders the atmosphere obscure—but I believe it went axially in the hydrogen (9274).

9254. *Ammonia* in Hydrogen. In this case the hydrogen had muriatic acid given to it before hand. The *Ammonia* was well and satisfactorily diamagnetic in the hydrogen.

9255. *Carbonic oxide*—is diamagnetic in hydrogen, more so apparently than Nitrous oxide—but then it is lighter and would give a more sensible effect on that account.

9256. *Carbonic acid*—was diamagnetic in the hydrogen a little but not as Carb. oxide (927[1]).

9257. *Olefiant gas*. Well diamagnetic in hydrogen.

9258. *Chlorine* was a little diamagnetic in hydrogen. It was clearly so, but the smoke might help the effect. I do not think they are far apart.

9259. *Mur. Acid gas*. I think diamagnetic in the hydrogen—but there was so much smoke present as to make it difficult to see. It was not much so.

9260. As the air of the globe is at different temperatures below and above, is it possible that through diamagnetic action this may have any influence on the relations of our atmosphere?

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9261. Further careful experiments as to the relation of air, hydrogen and Carbonic acid to each other—and first, of experiments in Air.

9262. *Hydrogen in Air* (9122, 3, 4, 5) was well diamagnetic to the air; there was no doubt about it.
9263. *Carbonic acid gas in Air* (9165, 6). The Carbonic acid was diamagnetic. The fall of the gas through the air and out of the lower side catch tube was very beautiful.
9264. *Nitrous gas in Air* (9175). It of course became Nitrous acid in its passage—and also became warm. It was however very slightly diamagnetic in the air, but the effect was so small that even the smoke might have done it.
9265. In *Carbonic acid gas* (9185, 9226).
9266. *Hydrogen* in C.A.—(9190) was clearly diamagnetic.
9267. *Air* in C.A. (9187). Went axially very well though not strongly, but it was fairly magnetic in C.Acid.
9268. *Nitrous gas* in C.A. (9228). As Air; it was magnetic in relation to Carbonic acid gas.
9269. A good platinum helix was arranged under the poles (9096), so as to give a stream of hot Carbonic acid in cold carbonic acid. A compound spiral thermoscope (9149) was fixed in a tube over the axial line, and either the spiral within, or the finger applied at the top of the tube, or both, could be used as indicators of the rising hot gas. The hot stream was strongly diamagnetic to the cold surrounding gas, for the helix, being retained hot, and the magnet not active, the lower end of the spiral made nearly $1\frac{1}{2}$ revolutions, and the finger above felt the heat; but when the magnet was active, the spiral returned back through these $1\frac{1}{2}$ revolutions and the air at the top of the tube was cold. On throwing off the magnetism, the thermoscope and finger instantly told of the renewed upright current of hot air (9230), (11186–92).
9270. In *Hydrogen gas* (9231, 9244).
9271. *Carbonic acid gas* in hydrogen gas (9256). The Carb. acid was diamagnetic, but not strongly when the hydrogen was good. Much depends upon the quality of the gas used as the surrounding medium.
9272. *Air* in *hydrogen* (9247). The air is magnetic to the hydrogen, being slightly drawn towds. the axial line when there is only little indicating smoke in it. When the air is shut off and a gentle stream of smoke descends from the end of the supply tube, that stream

is diamagnetic or repelled, and shews the effect of the smoke by itself.

9273. Repeated the experiment as regards the air in fresh hydrogen with the same result.

9274. *Nitrous gas* in *Hydrogen* (9253). This gas passes axially and is therefore magnetic to Hydrogen—but it does not go so strongly as Oxygen, or any thing like it.

9275. Then arranged the platinum helix in the hydrogen atmosphere, and the indicating thermoscope (9269) in the tube above it. But whether the magnetism was on or off, I could obtain no signs of heat in the tube above; i.e. though the ignited helix was directly under the tube and not more than one inch from its lower extremity, no heat ascended up into the tube at any time sensible to the thermoscope. This is probably connected with Grove's effects (8979), and deserves a close examination. Could the noses of the cold Iron poles do any thing?

9276. Continued the experiment until we had an explosion from the creeping in of air—but no harm done.

9277. Took up the French shade, and turning out the hydrogen, then replaced it full of air, and now when the helix was ignited, there were all the proper indications of heat, as before with the Carbonic acid (9269).

9278. Put *Coal gas* into the French shade with the platinum helix and then ignited the helix. Now there was indication of heat in the indicating tube above, and this heat disappeared when the magnet was rendered active, shewing that hot coal gas is diamagnetic to cold coal gas. But the signs of heat above before the magnet was on were by no means equal to those in air or carbonic acid. The coal gas seemed to be about half way between these and hydrogen.

9279. Threw *oxygen* into the French shade, and heated the helix in it. Here the effect was very good. The *whole* effect of the heat was a motion of $1\frac{1}{2}$ times or 540° of the lower end of the indicating spiral; on making the magnet active, there was a return thrgh. this whole quantity, so that then no warm oxygen rose between the poles. The hot oxygen was therefore diamagnetic to the cold gas. (11186–92).

9280. Then turned out the oxygen and replaced it by air. It

gave as nearly as I could perceive the same results and to the same amount. Hence as nitrogen makes up $\frac{4}{5}$ of air, hot nitrogen must be diamagnetic to cold nitrogen (11186–92).

9281. It will be seen by 9266 and 9271 that Carb. acid and hydrogen seem each to be diamagnetic in the other. This shews that they are close together. For the rest, if they were alike, the smoke would make the one in stream appear to be diamagnetic (   )–and further, if there were no smoke as in my apparatus, a little air will creep in and mingle with the atmosphere, so the gas which is as stream will be the purest and would seem to be diamagnetic in the other containing a little oxygen in the air.

20 NOVR. 1847.

9282. Let fall a stream of fine powder (silica from Fluo silicon into water) conducted by a funnel over and on one side of the axial line–but could not find any diamagnetic effect on it.

9283. So powders too coarse to imitate the stream of smoke particles.

9284. Hung a fine thread of cocoon silk by axial line–was not affected–mass is too small.

9285. Hung a slip of gold leaf there–it indicated distinctly, but was magnetic from the rouge, red bole, etc. about it gathered up in the beating and from the books.

Gutta Percha.

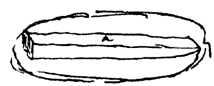
9286. Some of the soles are excellent electrics; they cannot be taken out of the paper, or touched by the hand, or any thing else scarcely, without becoming highly electric and affecting the electrometer as well as excited shell lac.

9287. Have been to the shop in Lisle Street and bought some thick sheet, very thin sheet, large band, small band and round cord. To my surprize, found a piece of the thick band would excite only feebly by flannel and could not compare with the former.

9288. Found also that, Electrometer being charged, if one end of the good sole was held in the hand and the other brought flat to the cap of the instrument, the electrometer leaves collapsed a little, to open out again when the gutta percha was removed–little or none of the electricity being conducted away. But on using a piece of the flat band (which did not excite) of the size of the sole nearly and applied in the same way–the leaves collapsed almost entirely, and yet on the removal of the gutta percha opened out as much as before, no electricity being really conducted away.

9289. Suspected this was due to the presence of water in the inner part of the strap piece, and so kept it in a warm place for several days; yet still produced no improvement of its qualities.

9290. Then cut two pieces from the edges of the active shoe sole and inactive or unexcitable piece of flat strap, so that the upper and lower surfaces were fresh and cut from the inner part. Excited the electrometer, laid these on and then applied the finger to the part at *a*, pressing a little. With the excitable gutta percha, there was no conduction and no discharge; but with the other, conduction and discharge instantly took place.



9291. Thus it was clear that the inner part of the strap was conductive but the exterior not, being dry, and this shews the reason why the induction was transferred on from the cap of the instrument thrgh. the middle of the strap piece to the part held by the hand–just as if the strap had had an inner plate of metal there.

9292. Took a piece of this badly insulating gutta percha–warmed

it and kneeded it between the fingers for a while and then made it into a ball and small rod, etc., all of which now insulated excellently well—as well as the shoe sole. So no doubt there is water inside.

9293. Heated some of the moist gutta percha in tubes in an oil bath up to 320° and 400°. Much water came off from both—and the pieces were left vessicular. Heated a little up to higher temperatures, until vol. substance came off, and took 2 or 3 different portions—the first, second and third portions being solid when cold, but the last a little tacky.

9294. All these conducted at first, but being dried a little to dissipate water from the vessicles, they improved, but were not equal to that which had been worked between the fingers.

9295. When the cut surfaces were examined, those exposed by the non conducting Gutta percha were more compact, resinous and had more lustre than those of the broad strap or non conducting gum. By a glass, the surface of the latter looked dull, as if impregnated with water.

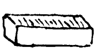
9296. In the soles and straps, as they are worked: if cut, they often present an external compact transparent part investing an internal opaque part which probably is hydrated and conducting.

7 FEBY. 1848.

9297. Pieces both of the thick and the very thin being put into water to soak for five days, and then taken out, wiped and examined, were found after a few minutes exposure to air to insulate and excite almost as well as corresponding parts which had been kept dry during the same time. So that when in good condition, it does not easily loose it by dampness.

8 FEBY. 1848.

9298. Portions of that which insulated well (   ) were put into boiling water, and changed very much in shape, as from Fig. 1 to fig. 2, the lines on the top of fig. 1 shewing in what direction it had passed through the rollers in the manufacture and in what direction strain was upon it. These portions were boiled for 2 or 3 minutes and then taken out and dried and cooled.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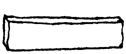
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9299. Another portion of the same Gutta percha was boiled in the same water, and much moulded about by two pieces of wood, the endeavour being to wet it internally, if possible. It was then taken out, dried and cooled.

9300. All these pieces discharged the excited electrometer sooner than a piece of the same gutta percha which had been kept for comparison, and apparently from the effect of the moisture. But being preserved for 24 hours in a dry room, they had resumed their original insulating power, as if they had merely been affected on the exterior, and that by spontaneous drying had returned to its first state.

9301. Pieces of the gutta percha which did not insulate well were also heated in the water and changed in shape, but not quite so much as the former. They also seemed to become a little vascular, as if some volatile fluid had been used in their preparation and was now becoming vapour. These when cold conducted much as before and were not improved after 24 hours.

9302. Some pieces of this badly insulating gutta percha were heated in a current of hot air, pulled out, doubled up and kneeded between the fingers many times in succession and at last formed into rolls or cakes. These by such treatment had very much improved in insulating and exciting power, and were nearly as good as the best specimens.

9303. Hence it appears that in the manufacture, water or some other body has been included in the badly insulating gutta percha, which such treatment can remove or displace.

9304. If either the good Gutta Percha (   ) or that which has been rendered insulating by kneeding in hot air, be heated in a tube, they give off water, as the first thing, just before decomposition commences, and the water is considerable, being perhaps equal to $\frac{1}{20}$ or $\frac{1}{30}$ the weight of the Gutta percha.

9305. The Gutta Percha is probably a complicated Substance.

28 FEBY. 1848.

9306. Tried to obtain the expansion of air in thermometer bulbs as described to me by Plückner[1] in his letter to me of the 6th

[1] Julius Plücker: Faraday's spelling of his name varies; see, for example, pars. 9378 and 9382.

instant, but could not obtain any sensible effect with very sensible tubes. The Magnet was not in its best estate. At any rate, *if* there be effect, it is very small.

4TH APRIL 1848.

9307. About a fortnight ago I made some of Schönbein's mixture of Iodide of lead and starch paste, rubbing them up in a mortar—applied the yellow pigment on paper, and exposing it wet to the sunshine, found it rapidly darkened and discoloured, as he told me in his letter.

9308. The unused portion of the mixture I applied on cards and paper, and allowed it to dry; it has been kept from that time to this without any sensible change of colour.

9309. A strip of this coloured cardboard being exposed in the bright sunshine of to-day in the dry state, did not appear affected, shewing no change of colour, except that after 5, 10 or more minutes exposure, it appeared a little darker in tint—a trace of orange coming over it.

9310. But if the coloured cardboard was moistened with water before being placed in the sunshine, it darkened instantly, just as if the mixture had been freshly applied in the moist state. If the focus of a lens were made to travel across the wet paper, the solar effect was beautifully shewn.

9311. But the more curious point is this. When the dry coloured paper was exposed to sunshine as just now stated, it did not sensibly change, but being then taken into a dark room or the darker parts of a sitting room, i.e. *out* of the sun light, and being *then* moistened with water, instantly a darkening effect came over the wet place, as if the sun's rays were still upon it. This effect was not so great as that produced by the exposure wet in the sun's rays, and it also went off soon in some degree, but it was very striking. Exposure for a minute in the dry state to the solar ray was sufficient to produce the effect, but exposure for 5 or 10 minutes was better. When the paper had received this power, it might be kept for a shorter or longer time *before* the water was applied, but the longer it was kept the less the effect. Still, it was produced though the wetting was delayed 15 or 20 minutes.

9312. In a hasty experiment, the action of the focus of a lens

did not seem more powerful than the sun's ray alone, but perhaps the heat communicated had interfered.

5TH APRIL 1848.

9313. A portion of the coloured card which yesterday had been exposed to the sun light for half an hour perhaps—and then put away in a dark draw—was to-day examined by touching a part of it with a wet camel hair brush, and the darkening effect immediately appeared. So that the power of being changed on the application of water remains for 24 hours at least.

9314. As before said, the darkening soon begins to diminish and when the paper was dry some hours after, the place was of a brighter yellow than the part as yet untouched by water, and as bright as the original card before exposure to the sun's rays.

6TH APRIL 1848.

9315. Again examined the card referred to above. When moistened by a camel hair brush over a part not as yet touched by water and over a part which had been touched—the former turned dark, shewing that the power of change remained for 48 hours—but the latter did not alter, shewing that one touch with water brought about the whole and final change for the time.

9316. Perhaps the effect may be due to this, that the water, by giving transparency, by that allows a change of colour to appear, yet without a change in the chemical position of the matters concerned.

9317. Or it may be, that the light has produced a certain amount of change, which cannot go back again, and that water by an additional and after action, brings this change forwd. by an effect of its own; which would seem to indicate that the whole photographic effect consisted of two parts, one preceeding the other, namely the action of light and the action of water.

9318. Or it may be, that the ray force is transformed into such a condition that when water is present *and not before*, then it produces its effect. In which case the condition of the latent ray force—its dispersion or employment in some other way before wetting—or its re-radiation—would be very important in a physical point of view.

9319. I rather incline at present to the second view—though I wish the third may prove the truth.

9320. When paper rendered sensitive by sunshine to the after action of water is warmed before the fire previous to the application of the water, it is not much altered unless strongly heated and then it loses the power. A piece laid on the fender for 10′ did not lose the power; a piece held before the hot fire for a couple of minutes did. So heat destroys the peculiar state.

9321. When paper which has been placed first in the sun's rays, then moistened with water and stained, is dried and so becomes bright again (9314), such paper exposed to the sun's rays will a second time shew the same effect.

9322. The yellow paper immersed in oil, or absolute alcohol or ether and then exposed to the sun's rays, did *not change*, even though the focus of a lens sent on to it. When put in a similar manner into water, it changed instantly. So water seems essential.

9323. A piece of the yellow paper was first well dried by fire and then exposed to the sun's ray, but in this case it acquired *no* power of changing colour out of the ray when moistened.

9324. A piece of the yellow paper not dried—or a dried piece afterds. slightly damped on the back, when exposed to the sun's ray, both acquired the power of shewing the after effect of water.

9325. So water essential to the peculiar state. Is no doubt a change even from the first. So there seems to be no reason to
9326. suppose the third case (9318) occurs, but probably the first or second or both combined.

9327. A piece of yellow card had a drop of water placed on it and then the sun's rays collected into a focus and sent on to it. Not only did the part under the water blacken, but as the water steamed and sent off vapour, this vapour enabled the part outside the drop also to darken by the sun's ray. So that vapour of water effect[s] the change in light as well as liquid water.

12 APRIL 1848.

9328. A portion of the card exposed dry to light on the 4th and since kept in a dark draw (9313), being now wetted, immediately darkened.

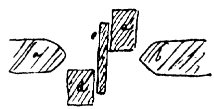
9329. Prepared cards with iodide of lead and proportions of starch var[y]ing from 20 to 1. All these when moistened and exposed to sun light acted well and nearly alike. On the whole, those with most starch the best: so that proportions do not seem important. The strongest was a stiff mucilage, and when with about half its weight of water, was added in such quantity as to form a thick fluid when rubbed up with the iodide, and then this was applied with a camel hair brush.

9330. Ordinary day light affects this photographic preparation, and especially the light of a sunny day—without the direct rays of the sun.

9331. When the Solar spectrum was thrown on to it, there was no discolouration in the red or orange rays, but it began in the green and continued up into the violet, shading off by degrees.

28 APRIL 1848.

9332. As to the mutual action (attraction or repulsion) of bismuth when in the magnetic field, *a*, *b* are the poles of my great Electro magnet with 20 pr. Grove's plates, *c* a bar of bismuth which vibrates well and quickly in the equatorial line, *d* and *e* two cubic masses of bismuth held near; but in no way of approach or position could I find any *sensible* influence of *d* or *e* over *c*. Hence if there be a mutual action, it is very small *under the circumstances*.



9333. As to *variation* of *conducting power* of iron when magnetic or not. Twenty feet of fine covered iron wire was doubled so as to make 10 feet and then made into a compact dense cylinder helix about a small glass rod, the doubled end being inwards and the two loose ends outwards—these therefore were the ends of two combined helices equal in power and contrary in direction, so as to have equal and contrary magnetic action. By two long wires this helix was connected with a delicate galvanometer and then placed between the poles of the great magnet. The equality of action in the two helices was such that no excitement of the great magnet or motion of the helix into and out of the magnetic field produced any current at the galvanometer. Then an electromotive element was introduced into the circuit so as to cause a certain permanent deflection of the electrometer, and the iron

double helix being between the magnetic poles, the magnet was made suddenly active or was suddenly rendered inactive, or the helix was taken out of the magnetic field. But no effect at the galvanometer was observed in any of these cases.

9334. At one time the Electromotive element was a very feeble hydro electric arrangement of zinc and platina wires in water—with the least acid—at another time it was a thermo electric element of platina and iron—but the same negative effects were obtained in both cases.

9335. Hence the iron helix did not appear to have any change produced in the conducting power of its metal, whether magnetic or not, or whether the helix were placed transversly or axially in relation to the magnetic forces.

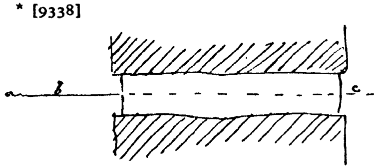
9336. It was curious to see incidentally how the wires of the galvanometer, when loose and lying about, could pick up current from parts of the floor or wood about or in the neighbourhood of the battery and magnet, when one would have thought that the connexion of the battery was perfect through the thick wire of the magnet.

9337. Tried the *tenacity* of various wires of copper silvered, Iron, etc., round and flat, when in the *intense magnetic field.* They all broke out of the place where they passed between the closely approximated poles of the great electro magnet, and hence there is reason to believe that when the metals cross the lines of magnetic force their *tenacity* is *not diminished.*

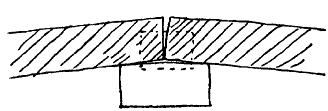
9338*. Arranged the two broad flat magnetic poles of the great magnet so that they faced each other, but had a piece of heavy glass between them; the faces were about 2½ inches long, and 0·6 of an inch wide. Reflected a ray from a small bright silvered bead so that it passed through the heavy glass and of course *transverse* to the lines of magnetic force. Then made the magnet active or through it out of action, the eye being at *a* to observe; occasionally polarized the ray at *c* and had an analyser at *b*, but under no variation of these conditions could I observe any effect of the magnetism on the ray.

9339†. The[n] brought the two magnetic poles together by their lower edges, except that a little soft cement intervened, but made them rest on a block so as to be out of the horizontal, which

* [9338]



† [9339]



made an angular space between them. When the ray was passed through this space close up in the angle, still no effect could be perceived.

9340. By soft cement fastened on a piece of mica at each end (see dotted lines), so as to form a trough, and filled the space with solution of protosulphate of iron. Then repeated the experiments, so that the ray passed through the magnetic solution, but could perceive no effect, except one due to a slight distortion of the arrangement when the magnetic force was on, which threw the image of the bead a little on one side each time. It was not a direct magnetic effect on the ray, but incidental.

9341*. Heated diamond by the oxy alcohol flame in the intense magnetic field. It burnt there and was affected by the oxygen just as if out of the Mag. field.

9342. Heated Plumbago and Gas retort carbon in the same manner—no particular effect. No appearance of their being changed *towards diamond.*

9343. Heated Plumbago and gas retort carbon wrapped up close in platina foil, in the Mag. field.

9344. Heated diamond wrapped up the same way and thought I could see in the specimens (three in No.), some appearance of approach to blacking or coke state.

9345. Must try this again, but do not think the Mag. forces have any thing to do with it. Perhaps diamond heated by oxy ether flame away from oxygen or air may become coke? Perhaps all diamond becomes coke before it burns away?

MAY 6TH, 1848.

9346. Fine Sunny day. Set up our large silvered concave reflector (2 feet in diameter) and experimented with a view to the *retardation* effect of the solar rays on *combustion*, etc.

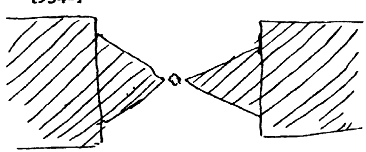
9347. *White paper* in focus heats slowly—quicker as it scorches and then soon ignites and flames.

9348. *Black paper* in focus fires very quickly and flames.

9349. *White touch paper*, with either nitrate of potassa or silver—nearly in same time as white paper but does not flame so readily—because less production of gas.

9350. *Amadou* lights at once.

* [9341]



9351. *White cotton match string*—slow because it is white, i.e. transparent, but soon fires.

9352. *Match rod of lignum* (brown sawdust)—fires.

9353. *Green and white taper*—wax rapidly melts—wicks rapidly heat and *ignite*.

9354. *Box wood charcoal*—quickly heats and *ignites*.

9355. *Sulphur*—on end of a match—quickly heats, browns and *inflames*.

9356. *Clean Spongy platinum*—becomes *strongly heated* in the focus—hence metal does not reflect all the rays, or would not heat.

9357. *Water in clear glass tube* became hot and soon boiled—hence glass and water do not entirely transmit all the rays—else they would not have heated.

9358. The focus of a glass lens 4 inches in diameter produced the same effects but not in the same degree. The kind of effect was the same.

9359. A small *jet of coal gas*, issuing from a glass burner, and when ignited giving a flame not above $\frac{1}{8}$ of an inch in length, was placed in the focus of the lens, and also in that of the mirror, but seemed in no way to have its combustion affected. When burning, it was not extinguished or diminished—when extinguished it was not lighted.

9360. A similar *jet of hydrogen* tried, with the same results.

9361. Iron inactive in N.A., when placed in the focus—gradually became active, but only by the heat communicated to the acid and tube by the rays.

9362. Thus there is no indication here that the solar rays have any influence in extinguishing combustion—but then the great heat communicated at the same time may have compensated for any effect of that kind.

9363. It is clear that transparent bodies do absorb a small proportion of the solar heating rays, just as they absorb a larger proportion of similar rays from a source of lower intensity.

9364. Sulphur on glass in mirror focus does heat and melt, but being melted, it runs about and does not heat much more and is not easily fired. Still, I could fire it. Also when inflamed, the focus did not put out the inflammation.

9365. Several equal lengths of white wax taper burnt in the bright sun light and in the shade—they appeared to take times as nearly equal as could be expected for equality of action.

9366. A bright ray was refracted by a prism and a good spectrum obtained. Combustions of white taper were effected in both the red and violet end of the spectrum, but both appeared to require the same time.

9367. A small jet of *coal gas* was not sensibly affected either in the red or violet end. When the red or violet or other rays were collected by a lens and thrown through the flame, the effect was still nil. A minute jet of hydrogen shewed the same indifference to the action of the rays.

9368. I can obtain no indication in any of these ways of an influence over combustion exerted by the rays of the Sun.

9369. Spongy platina, whether in or out of the sun's rays, at the focus of the mirror, or in the ends of the spectrum, has the same influence on a jet of hydrogen in air, in becoming hot or inflaming the gas.

9370. I have constructed what may be called a perfect discharging train by connexion of a continuous copper wire with the water pipes of our house.

9371. One end of my [illegible] Galvanometer wire was connected with this train and the other with a fine wire platina helix: then a gas flame was made to burn (a pencil of flame) so that it could pass up the helix or across it, and at the same time the rays of the *hot sun* from the concave glass silvered reflector be thrown on to it in all directions. There was no effect on the galvanometer—for the effects that did occur were traced to other and common causes.

9372. I put a ball of spongy platina in the place of the helix; still no effects.

9373. I connected a silver plate with the experimental end of the galvanometer wire—then iodized it in the dark—and then placed it in the sun's rays, and even in the focus of the reflector—but still no current or effect on the Galvanometer.

9374. After this, *both* ends (platina) of the Galvanometer wire were put into the flame, in different parts, the focus being in various ways sent on to them, but there were no signs of action.

9375. There seems to be no means here of procuring indication of current by *absorption of the ray* under these forms of experiment.

JULY 28, 1848.

Vibrations.

9376*. A thin rod of copper was fixed at the middle by a cork in a vice, being 4 feet long and 0·25 of an inch thick; and then a wet flannel being drawn over or along one end and pressed by the fingers, caused longitudinal vibrations and produced a high note. The middle *a* and the end *b* were then connected with a very delicate galvanometer, and the sounds reproduced, but there was no signs of any electric current produced, whether the exciting friction was at *c* or *d*.

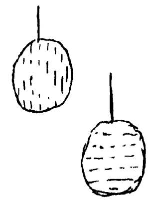
9377. A rod of iron of the same size was used in the same way. It gave a much higher note—but no signs of an electric current.

* [9376]

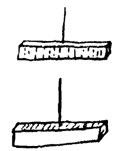
16 AUG. 1848.

9378. Plücker has described to me certain of his results as to the crystalline Diamagnetic relation, and as I understand it, the optic axis of a crystal having *one* optic axis tends to pass into the equatorial direction—or if a crystal have two optic axes, then the line between them tends to pass into the equatorial direction.

9379. He says, if a plate of calcareous spar be cut out of a rhomboid so that the optic axis is in the plane of the plate, and then the plate be suspended with optic axis vertical, it will be diamagnetic with a certain force—but if it be suspended so as to have the optic axis horizontal, it will point equatorially with a greater force.



9380. If a bar be cut out of this plate, so that the optic axis runs across the bar, and this bar be suspended horizontally, it will point equatorially—but if the same bar be turned 90°, so as to have its optic axis direction horizontal, then it will point axially by virtue of the optic axis force, notwithstanding the whole substance is diamagnetic.



9381. It seems to me that the optic axis force is most probably one of position, and not either of attraction or repulsion, and so is different to either the magnetic or the diamagnetic force. In that case, cubes or spheres of crystals ought to shew peculiar effects as to position or recession or attraction, either between broad flat poles or before a single conical pole.

9382. When the crystal has two optic axes, Plückner says the line between the two tends to go into the equatorial position. But I want to know whether, that line being in the equatorial position, the plane in which the two axes lye will take up any position, or whether rather it does not as a plane tend to the equatorial state and so both axes be equatorial.

9383. If that plane does not go equatorially, then as there are two lines between the two axes, which is the line that goes equatorially?

9384. Think of Brewster's well put case of the Glauberite, whose two optic axes by heat approach each other and then open out in a transverse direction—returning through all these positions as the crystals cool.

9385. Now when heavy glass or other bodies are magnetized and made to rotate light, the plane of no action of light is in the equator, and so corresponds to the optic axis of crystals, which are also lines of no action.

9386. Hence the phenomena are probably one in their general nature, i.e. the magnet *induces* an optic plane, whereas crystals have *naturally* optic axes; but both the optic plane and the optic axis have the same transverse relation to the lines of magnetic force and the same physical relation to the action on a polarized ray of light.

9387. And hence a beautiful relation of the magnetic forces and the forces which act upon a ray of light; or perhaps rather in this respect an approach to identity between them, for the forces which the crystal possesses naturally can be given by magnetism to glass, which is not crystalline, and even to fluids.

9388. Would it be impossible to modify the magnetic force effect in such a manner as to produce a body having one or two axes of polarization rather than a plane of polarization?

9389. If the magnetic result be viewed as due to a plane of polarization, then what a strange thing the plane of rotating quartz is, and still more, what a strange thing the lines or planes in rotating fluid present.

9390. When rotating fluid is in the magnetic field—what is the alteration in an equatorial direction? Does it lose rotating power more or less? It probably does—a good expt.

9391. Plücker says that the Mag. force and diamagnetic force decrease and increase in different ratios by their respective inductive actions. The diamagnetic force increasing and decreasing faster than the magnetic, as illustrated by his brass basin and mercury attached to the balance.

9392. In the first place, does a pure diamagnetic body ever become attractive or magnetic?

9393. Does a pure magnetic body ever become diamagnetic or repulsive?

9394. If not, then it is only mixed magnetic and diamagnetic bodies that shew the effect.

9395. And then the cause may be as Harris suggested at Swansea, namely, that the small mass of the magnetic matter, as iron by

approximation, becomes as it were saturated and can increase no more in power, or less than it did proportionately before, whereas the large mass of the diamagnetic body allows it to go on increasing in power in a more regular degree.

9396. Then the point would be, what is a mixed body? Magnetic brass is probably a mixture of brass and iron. But is oxide of iron a mixture of oxygen and iron, or is it essentially one constant magnetic body? Is also Sulphate of Iron crystallized one magnetic body or a mixture of magnetic and diamagnetic; and finally, is solution of sulphate of Iron a mixture or one body?

9397. Might by these considerations develope the cause, using crystals as against a solution—or green glass, etc. Whatever held a constant relation, either magnetic or diamagnetic, might be considered simple as to the acting substance—and whatever at different distances was both Mag. and diamagnetic as a mixture of the two.

9398. Plücker says that when bar of bismuth is in equatorial position—a bar of iron held under it makes it point far more strongly than before, and says this is due to the reverse states of the bismuth. May the effect not be referred in part to the alteration in direction of the lines of mag. force, i.e. are they not then weaker in the place of the ends of the magnet and so on? Perhaps a ball of bismuth would illustrate this point.

22 AUG. 1848.

9399. Referring to (9394) we may perhaps distinguish by the differing decrease of force between a metal, as platina, which has a definite weak amount of magnetic force, and an alloy or body weakly magnetic because it contains a few particles of strongly magnetic matter, as charcoal or brass.

9400. The optical condition in the magnetic field looks like a condition of particles, not of the masses. The natural rotating force of oil of turpentine looks also like a condition of particles; not of masses. The rotation of quartz looks like a condition of masses dependant on the associated condition of the particles. The position of a crystalline body, as calcareous spar, in the Magnetic field, must be due to its condition as a mass. Perhaps that may be questioned.

9401. Magnetic and diamagnetic bodies, when in the magnetic field, take on a *like* optic state. So that state is probably not due to the magnetic or the diamagnetic condition—*but distinct from it.* Think this well over. If so, it is very important—consider it especially in relation to opaque crystals, as bismuth.

9402. If oil of turpentine could be crystallized, would it rotate in *all* directions?

9403. Is there any *solid* body rotating the ray as oil of turpentine or other fluids do? Tartrates, how are they?

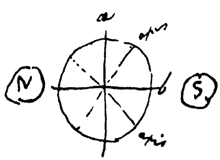
9404. What would be the position of a rhomboid of opaque calcareous spar in the Magnetic field?

9405. M. Plücker was with me again to-day and I enquired further of his conclusions, etc. (9378). He tells me that a bar of calc. spar with optic axis transverse to the bar and also horizontal, always points axially whether magnet be stronger or weaker, if it be strong enough.

9406. The Magneto optic force diminishes and increases less rapidly than the Magnetic force by change of distance—but is not altered in its ratio to the magnetic force by increase or diminution of the magnetic force, as by using a stronger or weaker current. This is a very singular result. How would it be with *one Mag. pole?*

9407. In the biaxial crystals, it is the line dividing the acute angle that seems to have the most magneto optic force.

9408. If axes be in a vertical plane, then that plane tends to go into the equatorial direction.



9409. If axes be in a horizontal plane, then the line bisecting the acute angle of the optic axes tends to go into the equatorial position. If there were 2 axes at right angles and the angles formed be supposed to be bisected by 2 other lines as *a*, *b*, then Plückner concludes that either the line *a* or the line *b* would be in a stable position if equatorial, the other being axial—and that if disturbed from these positions, the plate would move into one of them. The above diagram is in plan and the axes are supposed to be in a horizontal plane, the plate or crystal being free to move round a vertical axis.

9410. *Staurolite* has 2 axes and is magnetic.

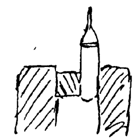
9411. *Tourmaline* has one axis and is magnetic.

9412. *Red ferro prussiate* has 2 axes, which he will describe to me on Friday next.

9413. A crystal of red ferro prussiate between poles of a certain strength is magnetic; then to shew its diamagnetic condition by virtue of its difference of diminution, the poles may be opened out or the crystal raised.

23 AUG. 1848.

9414. Made a weak sol. of proto sulphate of iron, put some into a glass tube, and sealed it up hermetically—put the rest to keep in a closed bottle for standard of comparison. Placed the tube and a cube of iron between poles of a strong horseshoe magnet, as in the figure, and have left it in the froggery, a place of equable temperature, to see if any concentration of the iron will take place below, at bottom of the solution. Purpose leaving it for a month undisturbed (9505).



25 AUG. 1848.

9415. To-day, Plückner shewed me for the first time some of his experiments.

First Optical results.

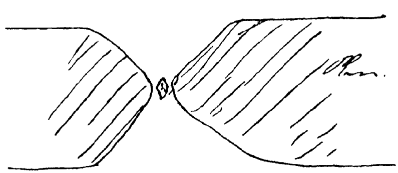
9416*. A small rhomboid of Calc. Spar was suspended by a single cocoon thread, between my Electro Magnet poles, with the optic axis in a horizontal position. When the poles were very close, as in the figure, the diamagnetic force of the substance made it take the position shewn, in which the optic axis is axial to the magnet. But when the poles were opened out to distance of half or three quarters of an inch, then the mass pointed axially and the optic axis therefore equatorially.

9417. When a larger rhomboid of Calc. spar, even larger than the figure[1], was suspended with optic axis horizontal, it pointed regularly and vibrated about a given position in which the optic axis was in the equatorial plane.

9418. There is a given distance between the Mag. poles (pretty close) when a certain rhomboid or piece of Calc. spar between them is so affected that the diamagnetic and the magneto optic force is balanced; at smaller distances the piece points diamagnetic

[1] Reduced $\frac{3}{4}$ scale.

* [9416]



–and at larger distances, Magneto optic. So that on increasing the distance, the magneto optic force diminishes *less rapidly* than the magnetic force, and on diminishing the distance it increases *less rapidly* than the magnetic force.

9419. *But increasing or diminishing the strength of the Magnet produces no alteration of this place of neutral action. It only increases or diminishes the strength of the action on each side of it; or rather, the resultant of the two actions on each side of that neutral position.* So Plückner at least tells me, for I did not see that proved.

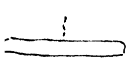
9420. Now diffnce. of Diamagnetic and Magnetic effect.

9421. A small piece of common Charcoal about this size[1], suspended by a single fibre of cocoon silk, when between the two Magnetic poles and they very close, pointed *diamagnetically*: but when the poles were withdrawn from each other, or the piece of charcoal raised above the poles, then at and beyond a certain distance, it pointed *axially or magnetically*. This is Plücker's illustration of different increase or decrease of magnetic and diamagnetic force; but in addition to this change by variation of distance, there is this further modification, namely, that increase or diminution of the *strength of the magnet* without alteration of the distance will convert the magnetic and diamagnetic results into each other, which *does not* happen in relation to the diamagnetic and the magneto optic force (9419).

9422. Appears as if diamagnetic force increased and diminished *more rapidly* than Magnetic force.

9423. As to charcoal, that is a very heterogenous body and may act like the hæmatite, etc. of Becquerel. How will a solution of iron act–or how will a metal wholly but weakly magnetic, as Platinum, act?

9424. Plückner now shewed me cases in which *Magnetic* and *Magneto optic* force were combined.



9425. *Tourmaline* has one optic axis coincident with axis of the prism. Being suspended horizontally, it was magnetic at all distances, shewn by its being *attracted* to either pole at all distances. Still, at a certain distance of the poles and at greater distances, or by a certain elevation of the prism above the two poles, it pointed equatorially; for at the same time that the center

[1] Reduced $\frac{3}{4}$ scale.

of gravity was attracted, the optic axis took an equatorial position. The effect is very good.

9426. *Staurolite* is a rhombl. quadrangular prism, having two optic axes; these are in a plane which passes through the obtuse linear angles of the prism. The red ferro prussiate of potassa is a similarly formed prism and also has 2 optic axes in the same plane. The optic axes do not form right angles with each other in either case, but the line dividing the acute angle which they form with each other and which Plückner finds to represent the maximum resultant of Magneto optic force, is in the Staurolite perpendicular to the mechanical axis of the prism, whilst in the red ferro prussiate of potassa it is perpendicular to that axis.

9427. Now then, as to *Staurolite*: it is at all distances magnetic, i.e. its center of gravity is attracted by the magnet. If the prism crystal be suspended vertically, when it can exert no *diamagnetic force*, the crystal vibrates strongly between the magnetic poles, the *plane of its two optic axes* always taking an equatorial position strongly.

9428. When suspended horizontally and the plane of the two optic axes vertical, then at near distances the magnetic force makes the mass point axially; but at greater distances the magneto optic power gains the ascendency and the plane of the two optic axes goes equatorially, carrying therefore the crystal into that position, and if there be any diamagnetic force, it will conduce to the same end. But if the prism be turned 90° on its axis, so that now the plane of the two optic axes be horizontal, the prism will as a mass now always point axially, and in this case the maximum resultant of Magneto optic force will *coincide* with Magnetic force to make the mass point axially.

9429. Now *Red ferro prussiate*. Always Magnetic, i.e. center of Gravity attracted. Magneto optic power very strong. Prism suspended vertically, vibrates about given position, i.e. with plane of 2 optic axes equatorial. When suspended horizontally, it always, beyond a certain distance of the poles, pointed as a mass equatorially; but perhaps, as Plücker says, more strongly when the plane of the axes was vertical than when horizontal. In the latter case, however, the maximum result of Magneto Optic force coincides with the length of the prism (and not with its breadth

as in the Staurolite), and therefore tends to place the crystal prism equatorial.

9430. By placing a sheet of mica over the 2 magnetic poles put very near together, and then putting some strong solution of Muriate of Iron or sulphate of iron on it, the forms assumed by the magnetic fluid were well shewn. When a diamagnetic fluid, as water, was used, the forms were the reverse to the former. They were much fainter but could be seen by reflexion.

9431. As to polarity of Bismuth. Plückner suspended a bar of bismuth opposite one *Mag. pole*, and then shewed that a magnet, or a piece of iron becoming a magnet, when approached, caused its motion with an appearance of repulsion—but all the effects appeared to me easily accounted for by the consequent displacement of the lines of magnetic force—and shewed nothing, as I think, that could not be resolved into the law of motion of a diamagnetic body from strong to weaker places of magnetic action.

29 AUGUST 1848.

9432. Have constructed an apparatus of the following kind. *a*, a tall glass jar standing on a wooden stool *b*, itself standing on a firm stone slab *c*; *d*, a large cork having a copper wire *e* passing through it, which sustains a single cocoon fibre *f*, from which hangs a triangle of fine copper wire *g*, which again by a single cocoon thread *h* sustains a large needle well magnetized, *i*, and also, to counterbalance it, a copper wire *k*. *l* is a card fastened to the bottom of the perforated stool, having a round hole in its center; through this a piece of straight glass tube *m* passes, the lower pointed extremity of which rests in a conical hole in a sheet of lead *n*. Three pieces of clean copper wire *o* are fixed into the upper end of the tube, to support any object required. The relative dimensions are given in the next page[1].

9433. Under these circumstances, the magnet *i* is free to move round the vertical axis *f* with an exceedingly small force. It is always vertical, or if the magnetic resultant is not vertical, still as it may rotate on the axis *h*, it can always have the same position as relates to the magnetic dip. The needle *i* in fact does vibrate

[1] I.e. in the diagram.

freely about a given position, shewing that its sides point, but under the circumstances, this does not prevent it from freely revolving round *f*, though it always presents the same side towards the North. Indeed it is theoretically possible (in consequence) so to arrange the torsion forces (feeble as they are) of the two threads *f* and *h* that they shall be in contrary directions and so the instrument be made of extreme delicacy; *in fact they are so arranged?* But nothing of this kind is necessary.

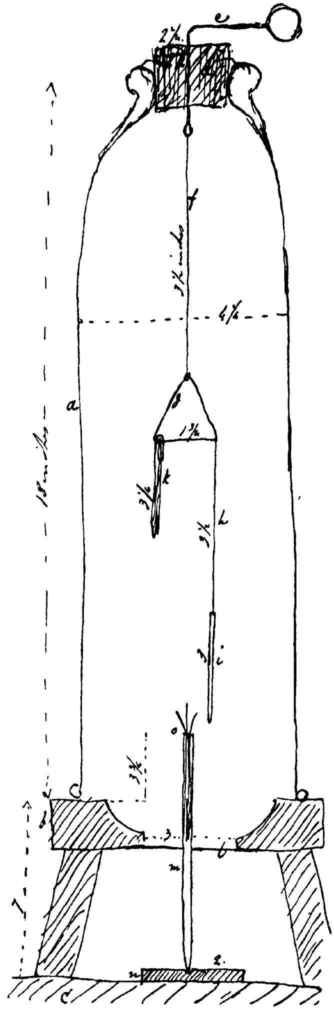
9434. By adjusting the glass rod *m* and its foot *n*, it is easily placed vertical, and then with any thing it may support is easily turned round on a vertical axis without disturbing the arrangement above.

9435. I found this apparatus very sensible yet very sure. It is in the froggery, a place of very regular temperature. If I leave it for an hour or two, it takes a certain position, easily read off by looking across from *i* to *k* to a graduation on the wall behind—and this it keeps from night to morning and morning to night.

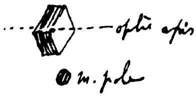
9436. But before observing, I have to strip my pockets of my knife, keys, watch, etc. and take a candle in a glass holder. Also I avoid stopping in long, as the warmth of the candle and self may by degrees affect the glass and so cause currents of air inside.

9437. Besides this, I find that in whatsoever position the fork *o* be, being revolved on the vertical axis, it does not affect the final position of the needle.

9438. A rhomboid of calcareous spar was now taken, nearly an inch in the linear edges, washed first of all in a little dilute Mur. acid and then in water, to free it from iron that might adhere to it, and then placed on the copper supports with the *optic axis vertical.* Being revolved thrgh. a few degrees, the needle was found to be affected sometimes—but this was traced to the motion of the air



displaced by the moved rhomboid, for when all was left to come to rest, the Magnet resumed the same position, whatever part of the rhomboid was towards it.



9439. Then the rhomboid was placed so that its optic axis was horizontal, and now the observations are going on. In order to record them, I will generally mark down positions in plan, as thus[1].

31 AUG. 1848.

9440. The Magneto optic force is not an optic force truly, or not an optic force dependant on the direct action of light, but a physical force existent in the crystal when no light is concerned—and also existent *before* and *without* the induction of the magnet. So differs from the Magnetic and diamagnetic forces, which *are induced* by the Magnet.

9441. Yet it surely must be the same as my optical force, though that certainly is induced. I mean the same in general character, not in its disposition.

9442. Then it would follow that both the magneto optic and the induced optic forces are in their most natural position when in the equatorial plane, for the one which is not induced takes that plane and the other, which is induced, is induced in that plane.

9443. The Natural Magneto optic force of a crystal therefore is in a constrained position when the optic axis is out of that plane, and if left to itself goes into it. Therefore if purposely moved out of it, it ought to react upon the Magnet or Electro helix. Hence may perhaps expect that its motion or position ought to induce electrical currents, and hence look for currents induced by the Optic force.

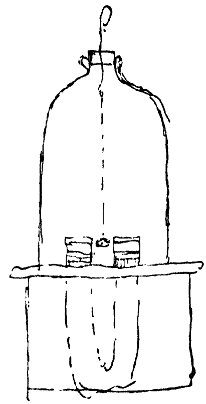
9444. The power must however be exceedingly weak; still, it may perhaps be found experimentally.

9445. Is it likely that the optic force of a crystal, as Calc. Spar, can induce optic force condition in contiguous bodies? Perhaps that a better mode of seeking for its inductive influence than by the generation of Electric currents.

9446. Suppose the Mag. axis vertical, and a *horizontal* optic axis prism, as a non magnetic tourmaline—or a plate of calc. spar, suspended at the side of the axis; is it indifferent, or what position will it take? That of a radius or a tangent?

[1] I.e. as in the diagram.

9447. The motion of the ends of the optic axis *seems* to be parallel to or along the lines of the magnetic force, and from stronger to weaker places of action. How to realise the reverse of this, so as to induce, if possible, currents?



9448. If the crystal (9446) takes a position with any degree of force, perhaps that may shew the best method of trying motion.

9449. Placed the smaller of our two compound horse shoe magnets upright in a box with a hole cut in the cover, and then placed a glass jar over the magnet. The jar was also supplied with a cork, sliding wire and single cocoon fibre suspender, so as to carry crystals, etc. between the poles of the magnet by a copper wire hook—which I afterwards found to be slightly magnetic, but not enough so to alter the character of the results.

9450. Obtained crystals of red ferro prussiate potassa from Morson; found them to be by this magnet as Plücker describes, Magnetic if between tip ends of the poles and so point axially, but when raised above them about $\frac{1}{2}$ or $\frac{3}{4}$ of inch, point equatorially.

9451. Whether the plane of the two optic axes be horizontal or vertical, the crystals when raised point equatorially, but more powerfully if the plane be vertical than if horizontal.

9452. When optic plane vertical (the length of the crystal being always horizontal) and the crystal axial and magnetic between the tips of the poles; if lowered between the cheeks of the magnet, then quickly and strongly points *magneto optically* or equatorially. The lines of Magnetic force are there more equable and then seem more favourably to affect the optic axis of the mass of the crystals better.

9453. The crystal, being first in the magnetic position between the tips of the Mag. poles, was then carried out sideways in a horizontal line. It quickly shewed the *Magneto optical* effect, and when half an inch or a little more from the axial line, pointed radially or equatorially and vibrated about that position. This continued to some distance.

9454. A smaller crystal was now used, a little shorter and not above half the diameter of the former, so not a fourth of it in weight.

9455. When lowered between the cheeks of the Magnet, it vibrated Magneto optically—quicker than the former.

9456. When raised to the place where the former was Magnetic, this was nearly neutral or magneto optic sluggishly. A little higher, it pointed well magneto optically; higher still, it was Magneto optic; higher still, Magneto optic—it was at this time an inch above the top of the poles.

9457. The optical plane was now turned into a horizontal position. The crystal now pointed well Mag. optic below between cheeks of Magnet—was magnet very slightly at the tips—a little higher became indifferent—and a little higher was Magneto optic, but not so strongly as when the plane of the two optic axes was vertical.

9458. That a small crystal may indicate better than a large one is almost a necessary consequence of the circumstance that, with a given magnet, the place is fixed where the magnetic and magneto optic forces change, in their joint resultant, from one to the other. The parts of a large crystal must be farther removed from this point than the parts of a small one, so a small crystal can better indicate the place of neutral or of maximum magneto optic force.

9459. Examined a crystal of Bichromate of Potassa in different directions by this magnet, but found nothing particular. There were indications, but not strong enough to make the crystal valuable to me.

1 SEPTEMBER 1848.

9460. Took a rhomboidal plate of clear transparent calcareous spar, being about half a rhomboid and perhaps $\frac{1}{2}$ an inch in its greatest length: slung it with greatest length and optic axis both horizontal. When below between horseshoe Magnet cheeks, pointed with optic axis in the equatorial plane very nearly and vibrated about that position.

9461. Took jar and crystal away from the magnet; placed it on the table and then crystal went round a circle and a half or more by torsion force of the suspender.

9462. Restored the crystal to the magnetic field; it immediately took position as before, but (as I intended) with optic axis 180° from former position.

9463. Placed the crystal now with greatest length horizontal but the optic axis vertical. The greatest length before pointed axially, but now it pointed equatorially, for as the optic force was thrown

out of action, the diamagnetic force became manifest. It vibrated about this position.

9464. When the greatest length was vertical and the optic axis horizontal, then the effect of the optic axis was evident, for it went equatorially. I found, however, that the copper suspender was very slightly magnetic, and that the force was (in the position) against the optic force. Discarded the suspender.

9465. After careful experiments continued for many days in succession, I cannot find that the magnet of the apparatus described (9433) is at all affected by the position of crystals on the support. A good transparent rhomboid of calcareous spar, as also a second rhomboid and a large crystal of red ferro prussiate of potassa have been used; with the former, the optic axis was horizontal, and with the latter, the plane of the two optic axes vertical and the line of greatest force horizontal, but whatever the position of this line in relation to the radius described by the magnet in its possible revolution—whether it were on the one side or the other—not the smallest difference occurred in the position of the Magnet, for it took a certain constant position dependant on the very feeble torsion force of the suspending cocoon filament. Hence this form not sensible enough to shew effect, which still theoretically is possible.

2 SEPTR. 1848.

9466. Have cast some cylinders of bismuth in small capillary glass tubes. Is easy to have them very small and clean and either in or out of the glass. The bismuth breaks very crystalline and often with one facet making the whole face of the broken extremity.

9467. One of these cylinders, about 0·2 of an inch long and 0·04 of an inch in diameter, was hung horizontally by a single cocoon thread (by a loop in it), and placed between the poles of the large magnet excited by 10 pair of Grove's plates, pointed *axially*, whether the poles were cones or flat faces. This was true either with one or two cones—from the smallest distance to six inches apart and more. When near the poles, it vibrated strongly about that position. When raised gradually above the poles to the distance of one or two inches, the magnetic poles being either near or wide apart, still the position was *axial*, and that strongly.

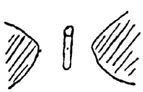
9468. Yet all this time the piece of bismuth was *repelled* either from one pole or from between the two—still though repelled it pointed axially.

9469. With the flat faced poles—the same effects were produced very strongly. These poles have two screw holes in the line of the axis, and when near to each other the bismuth cylinder tends strongly to go into these holes, either on the one side or the other of the magnetic field, but took up its place in the center of the hole and did not go to the sides, as it would have done if magnetic. Perhaps even possible to suspend a very fine cylinder by a very powerful pole.

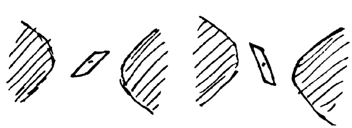
9470. The *crystalline fracture* was very oblique across the cylinder, and the planes of cleavage exposed were nearly vertical as the cylinder was suspended. Is not the effect a result of *crystalline polarity*, and is not crystalline polarity subject to magnetic force (cork model B 1)?

9471. A *second similar piece* of bismuth had exactly the same diamagnetic repulsion, etc. but it pointed *equatorially*, and was therefore in strong contrast with the former. It was crystalline, but the facets were not observed because the general effect and diversity was first looked for. The facets were nearly in the horizontal plane. See cork model B 2.

9472. A *third* larger piece of bismuth, in cylinder 0·5 of inch long and 0·1 of an inch in diameter (cork model B 3), with planes of fracture parallel at the two ends and oblique to the cylinder, was suspended horizontally; it pointed either equatorially or obliquely equatorial, as under the following circumstances depicted in plan. When the cleavage plane was thus:—so that a perpendicular to it was in the vertical plane, then the piece pointed equatorially; but when the cylinder was turned a quarter round, so that the cleavage was in the vertical plane, then it pointed about half way between the equatorial and the axial direction, and on the one side or the other according as it had been turned in the one way or the other; as the following forms shew*. These deflections are such as to shew that a *line perpendicular* to the cleavage planes tended powerfully to place itself equatorially, and therefore opposed the diamagnetic effect of the mass or combined with it.



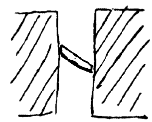
* [9472]



9473. When the square flat faced poles were used, either near or separated to $3\frac{1}{2}$ inches even, the effect was well shewn.

9474. Another piece of the bismuth, 0·75 of inch long and 0·1 in diameter, when placed in the suspending loop, was turned in different position[s] and examined. In one position it pointed well equatorially, as a piece of granular bismuth would do; but being turned round in the loop 90° (i.e., making what was the side of the cylinder the top or bottom) it pointed axially. Being examined, the end presented an oblique cross facet face, but there was also indication by a cross cleavage in this face that a bright plane of cleavage was running nearly along the cylinder, and also that when the bismuth was axial in position, this plane was vertical and in the axial line; so the result accords so far with the former results.

9475. A similar piece of granular bismuth might in certain cases take up an axial position between flat poles, because of the diminution of power from the angles of the poles inwards: but it was not so in this case. This piece stood thus, and if moved thus, it with great power turned thus; and this it did with either end to the right or left—shewing the effect of the structure of the whole mass.



9476. This action is very strong and may perhaps be sufficient for the generation of crystallo electric currents by induction.

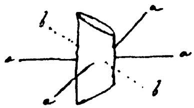
9477. Another cylinder, 0·6 by 0·1, was always nearly equatorial: and as to fracture at the ends, was small crystalline or granular—the crystals being in all directions. Good.

9478. Another piece, 0·6 long, was equatorial in one position in the suspending loop but axial if turned 90° in its bed. The planes were single at the two ends of the cylinder, and not parallel to each other: they were nearly at right angles to each other but so placed as to be both vertical at once.

9479. A short piece about this size, a little thicker at one end than the other, seemed to be crystallized uniformly throughout. The ends had cleavage planes as in the figure (and cork model N) which were sensibly alike set on the axis of the cylinder, and could both be placed vertical when the cylinder was horizontal. One was more acute on the axis of the cylinder than the other, and

they made together an angle of near upon 60°. When the line of intersection of these cleavage planes was horizontal, then the piece pointed equatorial*. But when this line and therefore the cleavage planes themselves were vertical or nearly so, then the piece pointed axially†. Here the line which nearly bisects the angle formed by the cleavage planes seems to be the equatorial line.

9480. Being suspended vertically as at N in the model or thus, then it pointed very strongly both between flat and pointed poles, and so that the headless pin represents the equatorial line. This line enters the cylinder nearly midway between two of the former points of suspension; thus, if *a*, *a*, *a*, *a* represent the four directions of suspension when the cylinder was horizontal, then *b b* represents the place of the equatorial line when the cylinder is suspended vertically.



9481. The cleavage planes of this piece of bismuth are marked with lines of other cleavage planes which are oblique on the two faces as respects the cylinder, but lie in the same parallel planes as respects the two ends. The plane of these lines nearly include[s] the line of equatorial direction when the cylinder is suspended vertically.

9482. Lest the little piece of wax used to suspend it vertically should be magnetic, and so give direction, the cylinder of bismuth was set round 90°, but it pointed just the same as before—and was always strongly repelled.

9483. Must procure some good crystals of bismuth—and also some well crystallized large cylinders.

9484. This must be crystalline polarity. But if ends of a crystalline particle or mass are alike, what a strange polarity it must be. The middle or equatorial parts of one crystal ought to repel either end of another.

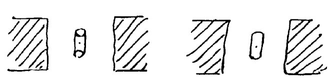
9485. On crystalline polarity of bismuth and other bodies.

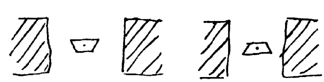
9486. Likely enough that a crystal of bismuth should point by the earth's action but should set either right or left.

9487. And that crystals should affect each other. Use spider web suspension.

9488. Perhaps even different bodies when crystalline affect each other.

9489. Ought to induce electric currents by crystalline force action.

* [9479] 

† [9479] 

9490. Repeated and extended some of Plückner's results.
9491. *Calcareous spar*—a clear transparent rhomboid between the flat poles, set well with Optic axis equatorial.
9492. I have three cubes of Calc. spar so cut that the optic axis in each is perpendicular to two of the faces. In all of them this line (the optic axis) pointed and vibrated well equatorially—the other two lines of direction were indifferent; i.e. when the optic axis was vertical, the cubes did not set to a position (9653).
9493. Two cubes of Rock crystal similar[ly] cut gave no sensible evidence of the magnetic force of the optic axis.
9494. *Flat magnetic poles* are good for these experiments—and small pieces of substance, for their diamagnetic force is nearly absent—the lines of Magnetic force being of nearly equal intensity.
9495. *Tourmalines*. Tried four pieces; the short thick pieces most striking, for then the magnetic force (of length) did not interfere so much. All shewed axial direction (magnetic) near to poles, and equatorial direction (optical) when more distant either above or sideways. All were bodily attracted.
9496. *Mica*—a prismatic crystal suspended horizontally very magnetic, but also pointed equatorially at a distance like tourmaline.
9497. *Pinite* crystal—always magnetic and axial.
9498. *Beryl* crystal—always magnetic and axial.
9499. *Arragonite* prism—diamagnetic in whatever position it was placed in the loop.
9500. *Felspar crystal*—nothing particular.
9501. *Red ferro prussiate of potassa* crystals—very good optic action.
9502. *Bichromate of potassa*—nothing particular.
9503. *Staurolites*. Have several—all good in their action—but the short thick ones are the best to shew the Magneto optical effect.
9504. The supposed northness and southness of bismuth in the magnetic field when iron is under it or near it (or a magnet) may be due to the disturbance of the lines of magnetic force by these pieces, and not to reverse attraction, etc. on the bismuth. All the facts seem included in the law I gave, that diamagnetic bodies tend to go from stronger to weaker places of magnetic action.

9505. Examined the solution at the Magnet (9414) by per ferro pruss. potash, taking parts from the top and bottom by a small pipette. They and a portion of the standard solution were sensibly alike in the quantity of iron present. Hence the magnetism had effected no separation or concentration in 14 days.

9506. There are no permanent diamagnetic bodies correspondant to a permanent magnet. A permanent diamagnetic body turned half round would be a magnet. This is a striking point in relation to the universality of the diamagnetic condition under temporary induction of a magnet or of electric currents, and makes a great distinction from magnets natural or artificial.

9507. On crystalline Polarity and its magnetic relations.

9508. A bismuth crystal should point by the earth's influence. If so, it would shew the crystalline force exerted at very sensible distances.

9509. Then there may be important terrestrial influences of this power. Consider its effect on mineral veins in the earth—and perhaps also on their direction.

9510. Has the cross tin and copper lodes of Cornwall any correspondant difference in the magnetic properties and relation of the contained minerals?

9511. Crystals of bismuth suspended near to each other ought to influence each other, standing either end on, side by side, but probably not thus $\top$.

9512. If so, would shew crystalline force exerted to very sensible distances. The power may be a repulsion end on and yet not sideways, $\top$ or —=—; a kind of equatorial repulsion on the poles of other particles when in a proper position.

9513. That the equatorial parts should repel *both pole parts* (in certain positions) is consistent with the regularity of crystals and their *two endedness*: in which they differ so much from cases of magnetic and electric polarity.

9514. If *two crystals* of bismuth cannot affect each other of their own force, perhaps they may when both are in the magnetic field—try them in different positions.

9515. Or perhaps bismuth and antimony crystals can so affect each other.

9516. Remember my jar of solution of Sul. Soda—crystallizing of a sudden in parallel plates through the mass of fluid ( ). Seem to have been predisposed either by the mutual action of the particles or perhaps assisted by the earth's force.

9517. Magnetism ought to affect crystallization. So fuzed bismuth placed in the mag. field and allowed to crystallize, ought so to crystallize that it would afterwards point in the same direction by the magnetic forces.

9518. If so, could obtain at pleasure a long piece which shall point either axially or equatorially. Or prepare a vertical cylinder, which shall vibrate about a given position.

9519. Prepare a globe of bismuth so. Would be a very good form.

9520. If this succeeds, it would perhaps supply a good mode of ascertaining the Magneto crystalline polarity of many metals and many other bodies.

9521. Try Tin—Lead—Zinc—Potassium—Sodium—Amalgams—Fusible metal.

9522. Phosphorus—Sulphur—Water—Spermaceti.

9523. Compare Magnetics and diamagnetics.

9524. Could fuze globules of platina—silver—gold—copper, etc. by a voltaic battery in the tense magnetic field and then let them crystallize there.

9525. Can crystallize solutions on Mica plates over the close poles—or in tubes small or larger.

9526. Can perhaps crystallize bodies here which otherwise would not crystallize.

9527. Can perhaps make sulphur take a particular one of its two forms.

9528. What of the optical properties of such crystals? Are they rotative like heavy glass, or axial, as an ordinary crystal?

9529. Which way will fibrous structures point, if at all?

9530. Which way will drawn rods and wires point?

9531. Is any electric current produced at the moment of polar crystallization? Try by a helix in the magnetic field—or by plates in a warm solution.

9532. Have two pieces of crystallized bismuth any electromotive power in certain positions of touch, as end and side touch—or

when in the magnetic field? Or have bismuth and antimony crystals any such power, being *cold*?

9533. *Calcareous spar* is much softer to file or grinding at ends of the optic axis than in other directions.

9534*. Perhaps to induct, a bismuth crystal should travel *along* the Galvanometer wire. Thus revolving the crystal on the axis *a* rapidly; for both ends have like power.

9535. Or if the bismuth crystal as a core were revolved in a helix, what would happen?

7 SEPTR. 1848. (Wimbledon).

9536. Bismuth crystals are said to be cubes. I think they cannot be so, but are either very obtuse rhomboids or oblique rhombic prisms. I tried to measure the inclination of the four faces in succession by a rough reflective goniometer, and obtained the following numbers:

88°		90°		88°
92°	again	92°	again	
88°		88°		
92°		90°		

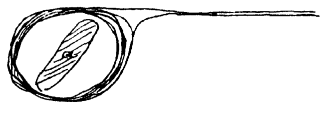
but much irregularity occurs in the planes and they are difficult to observe.

9537. When the crystals run on attached one to another, as with Mur. Ammonia, then the line of crystals is, I think, generally in the direction of the long diagonal of the rhombic faces.

9538. I tried also by a Haüy's goniometer to measure the angles made with each other by the four boundary lines of a rhombic face, and from an average of many made the acute angle 86° and the obtuse angle 92°. There is a loss of 2° here.

9539. Now worked with pieces of bismuth broken out and selected from the masses above referred to; taking pieces which, though they contained several crystals each, were still apparently crystallized uniformly, both as indicated by the consistent position of the crystals and also by the planes of cleavage, where exposed, running across or through the whole of a piece. The magnet was our largest horseshoe magnet, placed vertical, so that axis of force was horizontal, being arranged as at (9449) with a covering glass jar, and having the vertical cheeks of the magnet, bounding the

* [9534]



magnetic field, brought nearer occasionally by the introduction of a soft iron cube or plate. The width of the magnetic field from pole cheek to pole cheek was at different times from 1½ inches to half an inch. The poles were covered with clean foolscap paper, to prevent any contact of the suspended pieces with their sides.

9540. A piece of bismuth weighing about 10 grains, now fixed on a cork and called No. 1, was suspended by a single cocoon thread and placed between the magnetic poles. It vibrated well about a given position which it at last assumed. If the piece were turned horizontally thrgh. 180°, then it came into another position which it equally kept. When this happens, I will speak of the new position as the diameter position, to shorten words. Then a point facing one of the magnetic poles was taken as a new point of suspension, and it again vibrated in position. If now this piece of crystalline matter was subject to a line of force like Plückner's optical axis, that line should be at this moment in the intersection of the equatorial and the horizontal planes, i.e. an equatorial horizontal line. But on now suspending the piece in the third direction, so that that line should be vertical and so without influence, the piece still vibrated and in either of two diameter position[s], and also pretty strongly. It appears therefore not to have one equatorial axis of setting force (9600).

9541. Is it of necessity that the line of setting force, or the force, must be in the equator? Why may it not be axial, not only here but even in Plückner's results? The condition of one optic axis in the equatorial plane (as in Calc. Spar) is not very conformable to the simplicity of nature's laws.

9542. The piece set very well down between the cheeks when they were the whole distance of 1·4 or 1·5 inches apart (9567).

9543. *Piece of bismuth* No. 2, on cork, being experimented with in the same way as the last (9540), acted in the same manner, shewing polarity but not an equatorial line of determining force (9568); weight 11 grs.

9544. *Piece of bismuth* No. 3, on cork, consisting of many well shaped crystals and weighing 4 or 5 times as much as either of the others (65 grains), was tried in a great number of position[s], until one in which its motions were very sluggish and exhibiting very little polarity or setting force was found. The vertical line

was then marked by a little piece of cement, and then the piece suspended so that this line was horizontal; it then pointed well, vibrating and setting strongly; but the cement faced a pole, either one pole or the other, and so this seems to be the directing line of force, and in the magnetic field it takes an axial direction. Repeated this experiment many times with this piece.

9545. This line is the intersecting line of one of the solid angles of the crystals of bismuth, so that this angle points directly to the magnetic pole on either side, for the diameter positions are convertible here; and this angle appears to me to be that which is most acute.

9546. *Piece of bismuth* No. 4, on cork, not so large as the last but in fine crystals, had one of the solid angle[s] removed and replaced by a small cleavage plane, so that it could be well distinguished. The piece weighed ? grains. Placed it with this angle upwards—the piece was nearly indifferent as to polarity. Placed it so that the line perpendicular to the cleavage plane was horizontal; now it pointed well, the line going into the magnetic axis and the cleavage plane facing the magnetic pole. Keeping this line horizontal, the piece was rotated on it 90°; it still pointed well, and the line and facet in the same direction as before, in both diameter positions. So the directing force seems to be in a line or axis, and that not equatorial in the magnetic field, but correspondant with the magnetic axis (9608).

9547. *Piece of bismuth* No. 5, on cork. I placed this piece in three successive positions, so that the three pairs of rhomboidal or cuboidal planes should in turn be horizontal. In all the cases it pointed, the diameter line passing through the acute angles of the faces pointing axially and well. Then the piece was suspended with the acute solid angle pointing horizontally, when the piece pointed axially and stronger than in any other position. Lastly, the acute solid angle was placed upwards, and now, though the piece took a position, still the force of vibration or pointing was very small by comparison.

9548. *Piece of bismuth* No. 6, on Cork. Weighs 55 grains, presents good crystals and several cleavage planes. With the acute solid angle well vertical, it did not point at all. With the same angle pointing horizontally, the piece vibrated well and set, the acute

angle pointing axially. When the piece was turned round 90° on the axis of this angle, it still pointed as before with the acute angle axially, but the one position had more force than the other. Perhaps diamagnetic force assisted on[e] more than the other because of the shape. But the direction of the setting power is axial here as in all the other cases (9609).

9549. *Piece of bismuth* No. 7, on Cork. This was a fractured piece broken out of a large mass of well crystallized bismuth. It was roughly a square shaped plate, about $\frac{1}{8}$ of an inch thick and $\frac{3}{8}$ long and broad. The sides of the plate were the large cleavage planes that are obtained most easily and of the greatest size in breaking up bismuth, and though there be other bright cleavage planes, yet these, from their size and brightness, are usually readily distinguished. When this piece was suspended with the large planes horizontal, the bismuth scarcely pointed at all. But when they were in a vertical position, the piece immediately turned until they faced the magnetic poles, so that the line perpendicular to these faces was the line of setting force and an axial line. These faces manifestly correspond to the little facet on the top of the solid angle mentioned before, (9546) piece No. 4. (9610, 9666, 9668).

9550. *Piece of Bismuth* No. 8, on a cork. This piece corresponds to the last—and acted like it in all respects. When the planes were vertical and the silk thread clear from torsion, it required $4\frac{1}{2}$ revolutions of the index above, to which the cocoon thread (7 inches long) was fastened, before the torsion was enough to carry the plate round by the pole (9612). Weight 21 grains.

9551. Power evidently axial here.

9552. *Bismuth* No. 9, on Cork. Also a fragment piece, having from the mode of its division six chief cleavage planes and the form of an oblique rhombic prism. Two of the planes are bright and good, and I thought the axis of force would come out perpendicular to them, but these planes did not face the poles when vertical, and when horizontal, the piece was not indifferent, but pointed fairly. Found by trial the maximum line of force, and then found a small cleavage plane (which is now vertical in the piece on the cork) which represents the plane perpendicular to the line of polar force, and which plane always faced the magnetic pole.

9553. The line of crystal force seems to be in the direction of that solid angle at which there is the chief tendency for the crystals to run on in the dendritic form; perhaps it is the cause of that effect.

Antimony.

9554. Now wrought with antimony. Broke up a large piece, well crystallized within, and selected a fragment which seemed uniform in crystallization—but it pointed more or less in all positions (9630). As, though looking simple, it might be compound, I selected some small plates about $\frac{1}{3}$ of an inch long, $\frac{1}{8}$ wide, and perhaps $\frac{1}{40}$ or $\frac{1}{50}$ thick, which looked very regular in structure.

9555. These all pointed with the flat faces of the crystals towards the magnetic poles (like bismuth); only when the length was horizontal, the diamagnetic force was added on to the directive polar force. When the breadth was horizontal, this was not so, and then the crystalline polar force alone acted; and always well, taking 5 or 6 torsion turns of the cocoon thread to overcome it.

9556. When the flat faces were in the horizontal plane, the directive force was very small indeed—it was due perhaps partly to diamagnetic force and partly to other causes.

9557. Tried four plates of antimony, and having found that all were alike in pointing with their faces to the magnetic poles, I put them together symetrically with a little soft cement, so as to make the breadth greater in the direction of the line of force than in the cross direction, and so neutralize the force of diamagnetism. This combination pointed well Crystallo magnetic axially, and so confirmed all the other observations (9614).

9558. Both Bismuth and Antimony appear to have resultants of crystallo-magnetic force in the axial direction as regards the magnetic field. Plückner's resultant seems to be in the equatorial direction, so that the two forces appear to be different.

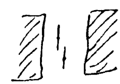
9559. Can they be in the relation of Pos. and Neg. resultants?

9560. My crystallo-magnetic action cannot be ordinary magnetic action, because the *masses* are always repelled.

9561. It appears to be a case of position only, and not of attraction or repulsion.

9562. Still, consider it well as a magnetic action with contrary poles opposite on the diameter line, to see what will come of it.

9563. If it is an action like ordinary magnetism, it must be *induced* at the time, for the pieces will point in diametrically opposite directions.
9564. If it is *not induced*, then it is a permanent condition, and then should produce mutual action between bismuths, antimonys, etc. etc.
9565. Should affect my hanging needle (9432).
9566. Will two in the Magnetic field affect each other?



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9567. Examined piece 1 (9540) again. Found one maximum axial resultant of crystalline Mag. force, but when hung with this line vertical, still another crystal apex pointed to the magnet forces, as if there were two or more minimum resultants of action—or as if the crystal as a group was complicated. Now on cork with strongest resultant vertical.
9568. Bismuth No. 2 (9543). One chief maximum axial line of power, but when this vertical, the piece still pointed, though weakly, as if a second resultant were present. On cork now with chief line vertical.
9569. The little piece of wax to which I attached the crystals is itself magnetic and points, and would shew some effect as with the crystals. Put on a new piece, but it also pointed—must be very careful of these things; it was not above this size.
9570. Every solid angle of the bismuth rhomboid may be replaced by a cleavage facet—so giving the octoedron—but the character of all the facets is not the same. That which replaces the most acute solid angle is brighter and more perfect, and also more easily effected, than most of the others, and is generally easy to recognise. It is that which bounds and is perpendicular to the crystallo magnetic line of force.
9571. *Bismuth piece* No. 11 on cork consists of a good set of crystals cut off by a good sectional surface plane. When this plane is vertical, then the line of force in a horizontal direction and perpendicular to its surface is a maximum. When this plane is horizontal, then the piece is as nearly as may be without setting force (9613).

9572. When vibrating with the line of force horizontal, the times of vibration are sensibly the same, whether the piece is in the midst of the magnetic field or close to the magnet cheek on the one side or the other, being always below within the cheeks (9542).

9573. Piece of copper wire. No signs of set in any direction.

9574. Piece of grain tin cut off from a long semi-crystalline part of a block of tin—no signs of any thing.

9575. *Antimony*. Three pieces well crystallized in cubes or cuboids. All acted very sluggishly, and gave no distinct maximum line of force. Also did not revolve in the magnetic field—and sometimes seemed to have a set which, by moving the pieces round in the magnetic field, could be changed into another position.

9576. Can this be due to any iron, and the assumption and retention for a while of the magnetic state? Are the pieces magnetic to a needle? Also try them by the Electro magnet.

9577. Try small plates of antimony by the magnetic needle. Try all the antimony and bismuth pieces so.

9578. As to the antimony's, must first try by the large magnet whether the pieces are magnetic; and next their crystallo-magnetic power. For they are much weaker and I think there is much difference between different specimens of antimony—perhaps from iron.

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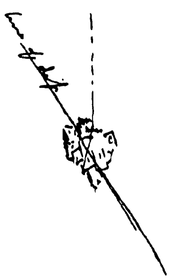


9579. Suspended bismuth No. 6 by a cocoon line 12 inches long in a glass jar, to see if the earth's power alone would affect it and give it a set, and for this purpose so suspended it that the line of force was with an inclination about that of the dip. So when the piece revolving by torsion, the line of force would describe a cone, in one part of which the line of dip would be. Observed to see whether the piece would settle with the line of dip in the surface of this cone, as that was to be expected, if the earth exerted any sensible action.

9580. Could not find that it did, for the bismuth when at rest was not in this position; but when the index was moved so that the torsion of the thread should take it there, it went there, and if the index was revolved a few degrees more, the piece by torsion of the thread obeyed. So that there was no sign of any effect

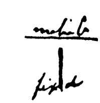
with this length of thread and this sized piece of bismuth. The thread was of such torsion strength that it took a minute to make a half vibration, i.e. either ↷ or ↶.

9581. Probably Arsenic a metal that will shew the effect. I remember some anomalous results with it before (   ). Perhaps also Arseniurets—and Sulphurets—such as Blend, Galena, etc.

9582. If so, shall perhaps have to correct list of magnetic and diamagnetic bodies—and point out how this crystalline polarity may influence the results and how by placing the line of force vertical it is to be distinguished. In this respect re-examine Arsenic, Platina.

9583. Suspended the bismuth piece No. 6 (9548), so that the line of force was horizontal—the suspension film being the same as before (9579) and 12 inches long. Placed near it the piece No. 3 (9544), about $\frac{1}{8}$ of an inch off, having its line of force horizontal also and in the same plane with that of the suspended piece. The suspended piece vibrated by torsion force of the suspension thread, and as it happened, about the line joining the two lines of force, so that this seemed as if the two pieces influenced each other. Observed the times of a half vibration, i.e. of ↷ or ↶, and found it by an average of several to be 64″. Then moved the jar so as to separate the two pieces by more than $1\frac{1}{4}$ inches, and that sideways. The line about which the suspended piece vibrated continued to be the same, so that the coincidence above goes for nothing. The times of vibration were now only 58″, an effect which I attribute to the obstruction which the stationary piece in the former case presented to the motion of the air with the vibrating piece.

9584. Now placed the stationary piece $1\frac{1}{2}$ inch off the suspended piece, and so that the lines of force (crystalline) were nearly at right angles to each other, and allowed the vibrations of the suspended piece to fall to 0 nearly. Observed carefully its direction and then brought it up very near to the fixed piece and left it to take its final direction, the lines of force being now thus.

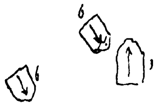


Well, after a time, both the vibrations and the final place of settlement shewed a displacement of the lines, for they became thus, clearly and distinctly.

Then moved the jar and pieces away from the fixed piece an inch obliquely and again observed

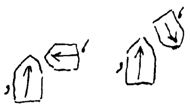
medium line of vibration and final line of rest. These were alike and thus, being a return to the former condition when two pieces were apart. So it seems as if there were an effect, but one which puts the two lines at right angles to each other rather than parallel. Try again.

9585. I think fine clean copper wire passed once round the pieces and cut off by a blow between coppers will be a better suspender than the wax before mentioned, for that picks up dirt from the fingers very fast (9569).



9586. The bismuth pieces 6 and 3 (9583) were at 9h. 50′ thus after one hour's rest–they were then separated from each other to about this extent; immediately the suspended piece 6 began to move–but whether from the cessation of the influence of 3, or from the effect of motion of air in the glass jar by the touch of hands, or by moving the whole, we shall see by the final settlement.

9587. This settlement was different to former position, for 6 had turned so as to set its line of force nearer to a right angle with that of 3, as thus. But I think the pieces are too near the surface of the table, and are perhaps affected by the currents formed in the jar from warmer or cooler sides and sweeping over it.



9588. Brought 6 up to 3 thus and then it swung round right handed into this position, after which it gradually swung back to ↑↙, and then went through following changes: ↑↖↑↑↗ ··· ↑ ↑ ·· ↑↗ ·· ↑↑ and even on to ← ↙ ↓, shewing that there were currents or other varying forces in the jar influencing the piece 6. It then went back.

9589. Now moved the Jar, so as to carry 6 $1\frac{1}{2}$ inches away from 3, to see how it would not be affected or set; was thus ←, and became in succession ↓ ↘ → ↗ ··· → ↓ ↙ ·· ↓ ↘ → ↗ ··· → ↓ ·· ↘ → ·· ↘ ↓ ·· ↘ → ·· ↘ ↓ ··· Put my hands to the jar to touch it as in moving it, but did not move it; now ↘ → ↘ → ↘ → ↘ → ↘ regularly, so that the warming alone did not cause much disturbance. Now moved the jar as much as was done in approaching or receeding to or from 3, but that did not disturb the oscillations of 6 sensibly ↘ → ↘ → ↘ → ↘ →.

9590. Now moved the jar and brought the piece 6 close up to the fixed piece 3, whose line of force was thus, and found it strongly affected through the following changes ↓ ↓ ↙ ↙ ← ↖ ↑ ↗ → ↘ ↓ ... ↘ → ↗ ↗ ↑ ↑ ↖ ... ↑ ↗ ↗ → ↘ ... →, etc. etc.

9591. It is probable that the bismuth 3 and its cork deflects and turns up the currents of air moving over the bottom of the jar or table and so affects 6.

9592. The table also is not quite steady, and there is a continual pendulum vibration of the suspended piece 6.

Must raise 6 and 3 into free space, and have a steady table and an equable temperature.

9593. If a body like piece 6 is swinging near a fixed air obstructor as piece 3, it is easy to see that the repeated oscillation may tend to set 6 round, for the resistance of the air as it goes up and its counter resistance as the swing returns, are not the same, and so a rotative force may arise. That should not make it take up a different final position, but then if there be currents of air across the bottom of the jar, however slow, they may have a different set on different sides of the piece 3 and so give different final positions of rest. Must keep 6 still and advance or receed 3 slowly.

9594. Being left for some time, the two pieces were thus, and very steady; then simply moved the jar of[f] in direction of the line *l* till the pieces were about $1\frac{1}{2}$ inches apart. Piece 6 swung (as was to be expected) thus, ↗ ↑ ↖ ← ↙ ↓ ↘ with irregular impetus, and then on still →, as if currents had been formed in the jar—then ↗, and having gone through more than a circle, it returned → ↘ ↓ ↙ ↙ ←; here a stand occurred, and then an impetus on thus ↖ ↑, as if there were some new action or some point of instable equilibrium—then ↗ and afterwds. it returned ↑ ↖ ←, and then again it returned ↖ ↑, and now vibrated ↖ ← ↖ ↑ ↖ ← ↖ ↑ ↖ ← ↖ ↑ ., so swinging about this position ↖, which is just the reverse of the former ↘.

9595. It was now brought to its former position near piece 3, and now piece 6 ↖ was not much agitated by the movement, and there it continued as if held there, swinging like a pendulum but not oscillating on the vertical axis. It was as nearly as possible diametrically opposite to what it had been before.


9596. Went out and after $1\frac{1}{2}$ hours, found it still so. Carried 6 round into this position, and there it vibrated about and took up quite a different position to the former, namely*.

9597. Still, all this may be currents—but certainly the pieces affect each other, one way or another.

9598. If bismuth crystals do affect each other, then it is possible that screens of crystalline matter interposed may affect curious changes, something analogous to those of polarized light. Perhaps even at the horse shoe or great magnet, something of this kind may happen.

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9599. Experiments made at R. Institution with the great Electro magnet in confirmation and extension of those made before at Wimbledon.

9600. *Bismuth piece No.* 1 (9540) points powerfully in same direction as with horseshoe magnet—and that *diametrally*—between flat faced poles or cone poles (either the obtuse or the sharp cones)—when the poles are separated to any distance, if affected at all it is in the same direction—or whether the piece of bismuth be raised above the level of the poles over the magnetic field—also with diminished power of the battery, as 1 pair of plates instead of 10 pair—or with one Magnetic pole only.

9601. During all these variations, the bismuth is always repelled from one pole or from between the two poles—so is always diamagnetic.

9602. When repelled from the axial line, the line of crystalline force keeps a position parallel to itself—or when moved about a single pole and shifted from side to side, the line of crystallo-force appears to be strictly parallel to or a tangent to the resultant of magnetic force, i.e. to the magnetic curve lines.

9603. The piece had been suspended with its length horizontal and vibrated very strongly with its chief crystal apex towards either magnetic pole: and being turned 90°, but still with the length horizontal, it pointed in the same direction and with the same force. So there is no sign of an equatorial line of force, though the idea of an equatorial plane of power might still be entertained. When turned 90°, but so as to bring the length

* [9596]

vertical, the piece also vibrated with considerable power; one of the solid angles then in the horizontal direction pointing axially almost as strongly as before—and also diametrally. It was the solid angle opposite the greatest fracture that pointed thus, and when the linear angle joining this and the chief solid angle was vertical, then the setting force was great with this linear angle towards the magnetic poles. When the next linear angle proceeding from the chief solid angle was vertical, the setting force was less—and when the third line was vertical, the effect was very much reduced.

9604. It is a question whether the various crystals forming this group are, as they appear to be, really symmetrical, so that the lines of crystallo-force are all in the same direction—or whether a single crystal has only one or more lines of maximum force. Proceed to examine some of the other pieces first.

9605. The short piece of copper wire, and also the wax or soft cement used to suspend and attach these crystals, was well magnetic to this power—must dismiss this wax.

9606. Made a better cement with pure white wax, a few drops of oil of turpentine and a little Canada balsam. It was very good and nearly unexceptionable, being so weak in magnetic effect, if it had any, as not to interfere with results obtained from bismuth.

9607. Now repeated the experiments with Bismuth No. 1 (9600), except that I did not proceed to find the position of the third or nearly neutral line. All the other results were the same.

9608. *Bismuth, piece* 4 (9546). The line of crystallo force corresponding to the small facet angle was very strong—but a line correspondant to one of the other solid angles pointed also, when this the strongest line was made vertical and therefore coincident with the axis of revolution.

9609. *Bismuth, piece* 6 (9548). The line of maximum crystallo-force is the same as before—but when this was vertical, i.e. coincident with the line of suspension, the piece still pointed diametrally, etc.; shewing either two resultants of force, or that the chief resultant is not strictly in the line of the solid angle.

9610. *Bismuth, piece* 7 (9549)—a plate from fractured mass. The effects were here very good. When the chief cleavage planes were horizontal, the motion of the piece was very sluggish, though there

seemed some tendency to point—but when they were vertical, they pointed strongly towards the Magnetic poles—diametrally—and that at whatever edge of the plate the suspending filament was attached. When the cleavage planes were not quite horizontal, then the piece pointed, and so that the line perpendicular to these cleavage planes came into the vertical plane or meridian passing through the axial line. By inclining the piece this or that way on the suspending film as axis, I could make any part of the edge point towards the magnetic poles, for it was only [necessary] to bring it into the position of the highest or the lowest point in the edge and it then pointed axially, for the line joining these positions pointed axially. In this way the pointing line of the fragment could be made to coincide with any diameter that could be drawn across the large faces of the plate.

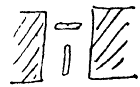
9611. I do not mean to say that the above observations are strictly true, but they are generally and apparently so. In one or two of the position[s], the maximum line of force seemed as if not quite perpendicular to the chief cleavage planes (9666).

9612. *Bismuth piece* 8 (9550)—a fragment plate like the last and presenting all the same phenomena.

9613. *Bismuth piece* 11 (9571)—group of similar crystals. The line of force here was, as before, from terminal cleavage plane through to the opposite solid angle, and very strong. When the cleavage plane was horizontal, then there was a little directive force, but not much.

9614. *Antimony* piece 10 (9557)—the block made up of four pieces. Suspended with length upwards—pointed powerfully with the planes of section to the Magnetic poles. When the cleavage planes are hung horizontal the mass points, but feebly. If the cleavage planes be inclined, then the same effect is obtained as with the bismuth plates before described (9610, 9612). The cement joining the four plates is the common soft cement—somewhat magnetic and had better be removed!

9615. *Antimony*—a new plate selected, now in paper and numbered 20, about 0·7 of inch long, 0·4 wide and not quite 0·1 in thickness. This plate presented new phenomena. Being suspended with the length vertical and in the magnetic field, it could when revolving without magnetism be caught by putting on the magnetic force,



just like a piece of copper (   ). If caught in either of these two positions, i.e. with its width either equatorial or axial, then on breaking contact at the battery it remains still. But if caught in any other position, there is *revulsion exactly as with copper*, and that on either side of the equatorial or axial lines—or in any way. If it be in one of these oblique positions when the magnetism is put on, then on making battery contact it moves with *increased force* up a little way to its fixed position—there it remains—until the battery contact is broken, and then the revulsion takes place. It is strongest when the piece forms an angle of about 45° with the axial line, and is, I think, as strong if not stronger than with copper.

9616. It will not in any position oscillate, but keeps the position it first took steadily—the advance is good—the revulsion better: perhaps if time had been allowed, it might have moved slowly into a given crystallo polar position, but being subject to this induction it cannot oscillate or move rapidly.

9617. So here signs of excellent *conduction* in a plane corresponding to the chief planes of cleavage and perhaps of no polarity—but what is the cause of the difference between this and other plates?

9618. This plate was now hung by the edge, but with the length horizontal. There was the *advance* and *revulsion* as before (flat faced poles were used), but there was also a tendency to set equatorially, and this may be due both to *diamagnetic force* and to the crystalline force, for then the cleavage planes face the magnetic poles. When the round conical magnetic poles were employed, then also there was *advance* and *revulsion*, but now the plate vibrated heavily once or twice and set well equatorially. The vibrations were sluggish and few, as if the plate were moving in a resisting medium. This is the effect of the induction in the good conducting mass—as with copper (   ).

9619. The piece was now suspended with the planes horizontal. Now there was no revulsion effect, which shews that there was no induction of currents in the plane at right angles to the cleavage planes. Probably because the plates are in a state equivalent to separation in that direction. It shews also that the circular currents induced are not round each particle, but travel through the mass: which further shews that this kind of induction is not

effective on particles but on masses—particles therefore may have quite another kind of induction, and even a contrary induction, as I have supposed in the case of bismuth (Exp. Res.   ).

9620. The piece now set equatorially slowly—not as if by crystalline power but by diamagnetic power. With pointed poles it set quicker equatorially. There does not seem to be much crystalline force here, but one might separate that and the diamagnetic force by cutting it up into vertical strips and then attaching these in the transverse direction, so as to form a bundle, and this would also do away with the inductive revulsive effect, and so bring out what crystalline power the antimony may have.

9621. The crystalline power is a power of particles. The inductive revulsive power is a power of the mass. The diamagnetic power is a power of the particles governed by the form of the mass.

9622. Is the direct repulsion from a pointed pole the same with a piece of bismuth crystallized, whether it is so placed that the line of crystallo force is in the axial or equatorial direction? It ought to be, if the crystallo power is one only of position, and also if the diamagnetic power is quite independant of it, and the same in all directions of the crystal.

9623. In all positions, this piece of antimony was repelled from a single pole or from the axial line, and so shewed the diamagnetic property of the metal.

9624. *Antimony*, piece 21 (9759) now in paper. This was a plate from a fragmented mass, good looking, with large flat good cleavage surfaces, about 0·5 of inch long, 0·1 or 0·15 broad, and about 0·04 thick—being much smaller than the former, No. 20. Being suspended with the length vertical, it vibrated well and freely, shewing *no signs* of *revulsion* or of retarded vibration. The cleavage planes faced to the magnetic poles as on former occasions (9555). It was then suspended at the edge, so that the length was horizontal and therefore the cleavage planes still vertical: it vibrated well, but not so quickly as before; indeed its greater length in plane of motion would prevent this. Still, the diamagnetic effect must be greater than before. The planes were now placed horizontal, and the piece now vibrated and set, but so slowly as to shew that though the diamagnetic effect would now be a maximum, yet the crystalline power being off, the effect was

thus reduced. The same effects were produced by pointed magnetic poles, and the piece was well repelled by a single pole.
9625. This plate therefore shews no revulsion, and strong crystallo force, but why are there these differences? Can this plate be in a state of division in planes perpendicular to the cleavage surfaces, so as to take off the conducting power in that direction and so prevent the formation of inductive currents? Or can some of the plates be Macles, so as to make them differ from others (9750, etc.)?
9626. The plates shewing the *revulsion* may indeed have the polar crystalline force, and yet hide it by the induced current produced by the motion the crystallo force gives, as said above; but the appearance is as if the crystalline force were gone or neutralized by counter position. Must examine this well.
9627. *Antimony, pieces* 22. Two other plates were now taken up which, though larger than the last, were in other respects and in appearances quite similar to it: but these now shewed the *revulsive* force, with all its consequences, just as piece 20 did (9615). The plates formed by the chief cleavage planes did not appear to be so firmly adherent to each other as the similar parts of 21, but I do not see how that could cause the difference (9750, 9756).
9628. *Antimony*, No. 23. Another smaller plate. It presents some revulsive action—but it also sets with chief cleavage planes facing the magnetic poles.
9629. *Antimony*. Other pieces, being plates, presented many varieties and degrees of these forces mixed together. It seemed as if such combinations were almost illimitable—but the crystalline force appears to be always there, and the point at present seems to be to make out the condition requisite for the *inductive revulsion effect*.
9630. *Antimony, Crystallized*. Have obtained three pieces or groups of crystals uniformly situated from the piece before spoke of (9554) and now examd. them by this Electro-magnet. As crystals, the forms seem to me to be more distinctly cubes than with bismuth; though here again some of the quadangular faces are rhomboidal, though bounded by well formed bright planes at the sides. The crystals are occasionally framed a little as with

bismuth, having the linear and solid angles complete but the faces not filled up; but this is by no means general as with bismuth, for generally the crystals are good solid cubes with very bright steel grey faces. There is a chief cleavage which takes off a solid angle and appears to be the cleavage presented by the fragmentary plates of crystals before referred to.

9631. *Antimony*, *No.* 12. A group of cubic crystals weighing 10 grains and having on opposite sides two parallel chief cleavage plane[s] passing across the cubic crystals as above described (9630). The length of the piece is rather more than its breadth or thickness, which are nearly equal. It was suspended with the length horizontal, and also the two bright cleavage planes horizontal; it then presented signs of revulsion but not strong or even marked; should not have seen them unless my mind had been prepared; it did not set or point, but took and appeared to keep any position. So no crystalline force evident thus.

9632. It was then hung with length still horizontal and the two cleavage planes vertical, and it now took a polar position at once with the cleavage planes facing the magnetic poles—consistent with the former crystallo cases of antimony. But in taking up this polar position, there was no vibration; it went up to it at once and kept it. I therefore conclude that there was inductive effect and the consequent formation of temporary poles, which would have produced revulsion, but that the crystallo force carried on the mass, causing the counter currents, which made its motion slower; and that when from this action it had taken its crystallo-position, it was just in the case where on breaking the battery contact no revulsion would be produced.

9633. The inductive revulsive action would be less because it could not occur in the whole as one mass, but in each solid crystal, the sum of the forces of which would be nothing like so great as if the whole had been one crystal.

9634. Can destroy the inductive and revulsive force even in copper by breaking up the large one mass into smaller.

9635. Now placed the length vertical, the cleavage planes being so also of course. It acted just as in the last case—going up at once to its place—not vibrating about that position—and that position being with the cleavage planes towards the magnetic poles.

9636. These are very beautiful results with the crystals of antimony and shew the crystalline polarity well.

9637. *Antimony crystals*, No. 13. A beautiful little group, in which the crystals form a rough pyramid having one of the solid angles of the crystals at the top, and opposite to this, at the other extremity of the piece, a cleavage plane, almost the only one on the piece; and this appears to be the chief and bright cleavage plane. Expected that the line joining the solid angle with the center of this plane would be the line of crystallo-force. Weight 12 grains.

9638. The piece was suspended with the apex vertical and therefore the cleavage plane horizontal. It was spinning rapidly, but on completing the battery circuit through the magnet, the rotation was instantly stopped; and on breaking battery contact there were signs of revulsion, but very feeble. The piece did not set and took any position, so that there were no signs of crystalline polarity.

9639. Then the line joining the apex and the cleavage plane was made horizontal and the cleavage plane was of course vertical: immediately the piece pointed, with this line in the axial position. However much the crystal was disturbed, it returned directly to this position. There was no appearance of revulsion (9632), but on the other hand, the piece vibrated as if in a thick fluid, and two vibrations would bring it up from its extreme position into its final crystallo position. *Good.*

9640. *Antimony crystals*, *No.* 14. The greatest length of this piece is terminated by the solid angles of the cubic crystals, but there are two bright and apparently chief cleavage planes parallel to each other and on opposite sides of the breadth. It was first suspended with the length vertical, the smaller solid angle being uppermost—the piece immediately pointed at once—making a dead set—with the two cleavage planes towards the magnetic poles. Then the piece was suspended with these planes horizontal—and now if revolving, when magnetic power put on, it stopped suddenly, and also gave some very small signs of *revulsion*, but it did not point and there were no signs of crystalline force here. Then hung it with the planes vertical and the length horizontal, and it pointed excellently well—there was no revulsion—but a firm advance and then a dead stop with cleavage planes facing the magnetic poles.

9641. These experiments with the cubic crystals are admirable.

9642. Must make out the difference of plates of antimony. Are they plates resulting from different sections or cleavages? Or are they different combinations from plates of two different sections?

9643. Can there be any effect analogous to those of polarizing and depolarizing plates in relation to light?

9644. Combine plates of antimony by quarter revolutions and then try the bundle.

9645. Would such plates of antimony produce any current of Electricity by contact of them sideways, either hot or cold? Remember Sturgeon's account of currents in a lump of heated antimony.

9646. Supposed it possible that, as a magnet affected a crystal of Bismuth or Antimony, so it might influence their particles when melted and cause the mass to assume a regular crystalline form on cooling. Took therefore some good bismuth—broke it up—selected some plates for comparison and melted others in a glass tube, which was then held between the poles of the Electro magnet until the metal had cooled and solidified. The piece as a whole (weighing about 150 grains) was then examined at the magnet, but had no crystallo-force, and no revulsive condition: it simply acted as a good diamagnetic body. On breaking it open, I found it to consist of an innumerable set of crystalline portions in all directions, if any thing even less regularly crystalline than if it had cooled under ordinary circumstances. Hence the reason why it shewed no *crystalline* or *revulsive power*.

9647. Hence also it appears that the magnet does not influence sensibly the arrangement of the particles in crystallization.

9648. The crystalline plates that were selected (9646) were found to possess both *crystallo-* and *revulsive* forces or condition. Hence another proof that these depend upon the mode of aggregation of the particles—for the same particles confusedly crystallized gave no effects, and had those it before possessed taken away.

9649. A solution of Nitre just at the crystallizing point was put on a plate of mica placed over the poles, they being very near together—but the magnetic force seemed to have no influence on the crystals that were gradually produced.

9650. Is the crystallo force one induced by the magnet, or is it

only a resultant of the forces which all the particles possess at all times and which are symmetrically exerted in a uniform crystalline mass, even to the production of external influences?

9651. That the Magnet does not induce regular crystallization (9646, 9649) looks a little like the former; and perhaps also the relative inactivity of the crystals of antimony when at the horseshoe magnet (9554) as compared with the results at the Electromagnet.

9652. When estimating the relative forces by the number of vibrations of such a body as well crystallized antimony, must remember the retarding induced effect if necessary, and account for it.

9653. Try the small cube of Iceland spar (9492) in very strong magnetic field to see if the line of maximum power is really the optic axis; i.e. whether that line is equatorial or axial.

9654. Have crystals either of bismuth or red ferro prussiate of potash any mutual action independant of the magnet? If they have, can the force act through glass or mica?

9655. If it can, it must be an independant force, i.e. one not induced; and whether it is independant or induced is an important question, and must be considered so as to distinguish and determine each.

9656. If the force can act through glass, metal, etc., that an important character of it, rendering it in some degree analogous to the magnetic.

9657. To make the mutual action results very sensible, put a reflector on the swinging body and observed place of a fixed light when reflected.

14 SEPTR. 1848. (WIMBLEDON).

9658. *Galena*—a small cube cloven out of a good and regularly crystallized piece, gave no signs of pointing or of crystal polarity by the horseshoe magnet (9539). It turned with the slightest torsion given to the silk by the index above.

9659. *Rock salt*—a small cube of this substance cloven out of a mass gave no signs of pointing or crystal polarity.

9660. The copper and wax attachments very free from magnetism.

9661. *Artificial Sulphuret of tin* in small prismatic crystals gave no signs of pointing or of crystal polarity.

9662. *Tourmalines.* The shorter thicker one acts well; its length is little more than its breadth. When the optic axis is vertical, the piece revolves regularly and obeys the torsion from above readily; there is no set or pointing. When the Optic axis is horizontal, then it points equatorially, and if the prism be turned on its axis 90°, still the optic axis sets equatorially and strongly so. So here there is no doubt that the maximum line of setting force is equatorial and coincident with the optic axis.

9663. The other tourmaline is longer and narrower, so that its length is about twice its breadth. When the optic axis is vertical, the prism revolves regularly and obeys torsion from above readily: there is then no set due to crystalline or magnetic force. When the prism is horizontal, then it sets equatorially, but it wants more magnetic power and perhaps distance to compensate for its proportionate increase in length.

9664. *Red ferro prussiate of potassa.* When hung horizontal but with the plane passing through the obtuse linear angles of the prism vertical, then it vibrates well and sets equatorially: with the prism vertical it still vibrated well and with the same plane equatorial.

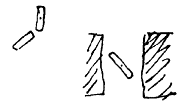
9665. One might therefore consider a line joining the acute angles of the prism as an axial line of power, only that when this line is vertical, the crystal still points; as Plückner states and explains in his way. And certainly, that line being axial, the crystal points far better if it be suspended horizontal than vertical; as if the maximum of directing force were equatorial and in the axis of the prism.

15 SEPTR. 1848. Wimbledon.

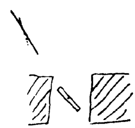
9666. Two pieces of bismuth, Nos. 7 and 8 (9549, 9550, 9610, 9612) were employed with the ordinary horseshoe magnet (9539). One of the pieces, No. 7, was suspended by a single cocoon thread between the cheeks of the thread[1], and the other, mounted on a moveable support, brought near it in various positions to see if the mobile one would be affected by the other, both being

[1] ? magnet.

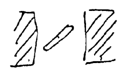
under the influence of the magnet. Both pieces were vertical and therefore their crystalline lines of forces in the same horizontal plane and as assumed perpendicular to the cleavage planes of the plates. The position of the plates and therefore of their lines may be represented in plan thus. Rather unexpectedly, No. 7 stood thus in the magnetic field, but as this was not expected to negative any action that might exist, the reason was left for future consideration.



9667. The piece 8 was then placed in all the following position[s]*; but without the slightest apparent change being produced upon the position of No. 7. It is true that the positions Nos. 7 and 15 seemed to set the piece suspended a little more round, i.e. from the longer to the shorter line, but the eye could not be trusted and I doubt it. Must repeat the experiments with a stronger power—the Electro magnet and better pieces of bismuth.



9668. Have said that piece No. 7 hung thus; besides, it would not point diametrally but always returned (9666) with one particular end towards the North pole of the magnet, as if it were magnetic to a certain degree. It is, generally, a square thick plate and has four edges, three of which are fractures and the fourth that which has been in contact with the melting vessel; it was suspended by this edge when it thus pointed. Now it was suspended by the opposite edge, and then pointed thus: it also would not point diametrally and the same end pointed to the North pole of the Magnet as before. The resultant line of force therefore cannot be perpendicular to the sectional planes, and the whole look very like a combination of ordinary magnetic force with the other forces.

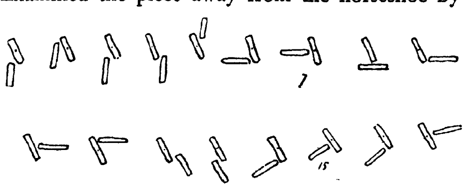


9669. Being suspended by the other two edges, the piece pointed with the cleavage planes nearly facing the poles and also diametrally, but it was sluggish in its movements, as if the line of force not in the best position.

When the planes were horizontal, the piece was not indifferent but pointed. Observed the direction and then turned it 90°, so as to bring the line of force (if crystalline) into the axis. This brought the suspension obliquely from the cast edge (9668), and now the piece pointed as at first  and *not diametrally*.

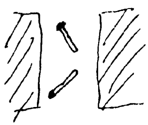
9670. Examined the piece away from the horseshoe by a bar

* [9667]



magnet, and now found feeble signs of magnetism, for one end was attracted and the other repelled—a state enough to account for the irregular effects. Rubbed off the cast edge by sand paper and suspended it again by that edge between the poles of the horseshoe magnet; it was better than before and could now set diametrally, but with constraint only, and the final position was very oblique.

9671. A thin plate separated from the piece pointed diametrally and with the planes to the poles: also it did not point when the planes were horizontal and then obeyed torsion force easily. The remaining part was split into two nearly equal plates—each plate could stand in two position[s] thus—but the same edge was always towards the same magnetic pole—neither piece could set diametrally whilst suspended either by the cast or by the opposite edge. These pieces are too irregular to preserve at present, so threw them away.



9672. Must select plates from the middle of masses. They present many varieties of combination of forces and it is very important to distinguish simple elementary plates from those of complicated crystalline composition or affected by magnetic or other forces.

16 SEPTR. 1848. (London).

9673. Used great Electro-magnet and 10 pr. of Grove's Plates for the purpose of examining Rock salt and a number of other bodies, not metals, as to any evidence of the Crystallo-power.

9674. *Rock salt*—a small cube of about 0·25 of inch in the side, cut out of a piece of well crystallized salt, perfectly pure and colourless, using a platina knife to cleave it; and on a representative cork model numbered the faces in pairs 1, 1′: 2, 2′: 3, 3′. When suspended with faces 1, 1′ horizontal, then when between conical Magnetic poles, faces 2, 2′ were towards the poles; but with flat poles, faces 3, 3′ were towards the poles—in either case diametrally.

9675. When planes 2, 2′ were horizontal, then planes 1 and 1′ faced to the pointed poles, and planes 3, 3′ towards the flat poles. When planes 3, 3′ were horizontal, then plane[s] 1, 1′ faced towds. the pointed poles and planes 2, 2′ towards the flat poles.

9676. Can this be diamagnetic action only, or is crystalline action

concerned? The greatest length is I think from 3 to 3′—the next from 1 to 1′—and the shortest dimension parallel to the sides is from 2 to 2′, but there is very little difference.

9677. A mere diamagnetic body 0·25 of inch in length would point equatorially with the conical poles but axially with the flat poles, for because of their shape and size and also because of a screw hole in the middle of the face of each, the lines of magnetic force are weaker in the middle of the space between them than in parts nearer the edges or boundary of the magnetic field.

9678. Struck a plate off from face 3, so as to make the length from 3 to 3′ rather shorter than from 1 to 1′, and suspending it with the planes 2, 2′ horizontal: then between pointed poles, faces 1, 1′ still pointed to the magnetic poles, and between flat poles it pointed diagonally, the angles 3, 1′ and 3′, 1 towards the poles. When truly between the pointed poles, it also set the *same way*—being repelled from the axial line at the same time on either side.

9679. Struck off another plate from face 3, so that now the line 3, 3′ is much the shortest. Made 2, 2′ horizontal as in the last case, and now 3, 3′ pointed to the conical magnetic poles, and 1, 1′ to the flat poles, which is just the reverse of the first positions with 2, 2′ horizontal. The greatest length therefore goes axial with flat poles and equatorial with pointed poles, as should be the case with a purely diamagnetic body.

9680. Hence no signs of crystalline or optic force in crystallized Chloride of Sodium.

9681. What great care is required in separating the phenomena of the different forces? The contrary result of the two differently formed sets of magnetic poles as regards diamagnetic force (9677), and their like action for crystalline or optic force, will give a power of distinguishing one from the other.

9682. Salt is excellently well diamagnetic.

9683. It is evidently possible to have poles so shaped in their corresponding surfaces as to give a magnetic field of sensibly equal forces, for a given and considerable space near their middle parts. These must be convex, the surfaces being probably portions of large spheres, but the sphericity would probably have to differ with the strength of the battery or magnet. A little bar of phos-

phorous or heavy glass or minutely crystallized bismuth would serve to examine such a field and also indicate the adjustments required to obtain it. Then in such a field the crystalline conditions and results could be observed in their greatest simplicity.

9684. *Galena.* A cube of this body, separated by cleavage from a good mass, gave no signs of crystalline power. There was plenty of repulsion and it is well diamagnetic, but no indications of either axial or equatorial force.

9685. *Fluor Spar.* A very clear piece, cloven out of a well crystallized portion of chlorophane, and carefully examined in various ways. It possessed a good diamagnetic action, but presented no traces of crystalline or of optical force, subject or related to the magnet.

9686. *Cinnaber.* A good crystal—ruby colour, transparent. Diamagnetic. No signs of any crystalline or equatorial force.

9687. *Sulphuret of copper*—a good crystal—points: but is magnetic.

9688. *Sulphuret of Zinc*—good crystals of Blende. All magnetic.

9689. *Cobalt Glance.* Good crystals. All magnetic.

9690. *Oxide of tin.* Good crystals, native. All magnetic.

9691. *Leucites.* All magnetic; easily made magnetic in any direction and then pointed and vibrated accordingly.

9692. *Red oxide of copper.* Good native crystals. A piece was altogether diamagnetic, but there was also irregular motion, sometimes like *revulsion* and sometimes like a little magnetism. A smaller piece was Magnetic. There was no clear appearance of crystalline force, and I do not think there was any.

9693. *Copper.* Good native crystal. If rotating, it stopped short, and then there was *revulsion.* When stopped, it did not move slowly up to a certain position, like antimony (9618), and indeed shewed no signs of crystalline force. Must try it with the horse shoe magnet (9715).

9694. *Arsenic.* Broke up a crystalline mass of the metal and selected a plate that seemed well crystallized; it was about 0·3 of inch long, 0·1 wide and 0·03 thick. When suspended opposite one conical magnetic pole, it was well repelled, proving to be diamagnetic: when between both poles, the faces of the plate pointed to the poles powerfully. This was to be expected, being consistent with its diamagnetic character and form: but on using the flat

faced poles, the diamagnetic phenomena were soon separated from those of a crystalline character, for then it also set well and freely with the faces towards the poles—and also diametrally. When the piece was thus suspended with the length horizontal but the faces vertical, it did not set axially as a diamagnetic body would have done, but equatorially, i.e. with faces towards the poles. When suspended by the edge with the length upwards, it vibrated very powerfully by comparison, setting with the faces towards the poles.

9695. When the piece was suspended with the faces *horizontal*, then it set variously and according to the inclination of the plate, a plane perpendicular to the plane of the plate always setting axially—just as with the plates of Bismuth and Antimony (9610).

9696. Another larger plate of *Arsenic* was taken which, though complicated in its crystallization, still appeared to have the constituent plates parallel. This piece presented exactly the same phenomena as the former (9694). Other plates did the same.

9697. *Arsenic* therefore has a strong crystalline power.

9698. *Lead.* A rhomboidal portion cut away from a mass that had been crystallized in the usual manner in a ladle—pointed feebly both with conical and flat magnetic poles, and in both cases the same way—the axial line being a *line of medium length* across it. But the lead is caught in its rotation as copper is, and also shews signs of revulsion. Hence if it have crystalline force, there can be no vibration, but only a slow and dead set. Now lead is rather a bad conductor of Electric currents, if I remember rightly, but this catching appears to shew that it is pretty good, and also pretty good in all directions; the latter I suppose indicates moderately equal contact *in all directions*, and also comparitive equality of polar force in all directions (9717).

9699. *Bismuth*—a cube cut out of crystallized mass, and the faces of separation finished off by sand paper, has on it a plane of cleavage looking like that which is perpendicular to the crystallo line of action. Hung it up so that this plane was vertical, and it instantly pointed to one or the other magnetic pole.

9700. Bismuth crystal No. 1 (9540, 9600) was suspended in the magnetic field with the line of crystallic action horizontal; it of course pointed powerfully. Then brought Bismuth crystal No. 5

(9547) near it with its crystallic axis also horizontal, and in various positions, but could perceive no trace of mutual action between the two bismuths.

9701. Held a crystal piece of *bismuth* in the magnetic field made very powerful by conical poles. Took it away, and instantly examined it by a delicate magnetic needle, to see whether any trace of sensible force remained in it. Could not find there was the least indication of its bringing away any state from the magnetic field.

9702. Suspended a *bismuth crystal*, No. 1 (9540), between the Magnetic poles retained at a fixed distance and observed its vibrations, as well as I could, by a small temporary pendulum. Then interposed as much as 1 inch in thickness of bismuth on both sides of it, by way of screens between it and the magnetic poles, and again observed the vibrations to see if any difference in the force upon it was produced. I could not observe any sensible difference, but it is not easy to make an accurate observation, because as the directive force diminishes as the crystalline axis makes an angle less and less acute with the magnetic axis, so the vibrations in large arcs are much slower than those in small arcs, and thus a cause of uncertainty is produced not easy to avoid, as the extent of the arc of vibration is not easy of regulation.

9703. As to words expressing the force, will not these do, being compounds of Magnet and Crystal? *Magcrystallic force*—*Magcrystallo* or *Magcrystalline* axis—or relations (9706).

9704. A crystal of bismuth in the magnetic field should be affected by a piece of iron—because the latter alters the form of the resultants of magnetic force. It might even be used as an indicator of the forms of the curves, not affecting them sensibly itself. I doubt Plücker's account of the action of Iron on bismuth (amorphous) in the magnetic field: perhaps we may here find the means of clearing up the doubts.

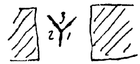
9705. Do irregular pieces of bismuth or pieces a little magnetic come out in relation to their Magcrystallo phenomena better with stronger than with weaker magnetic force?

9706. Better probably to say *Magnecrystallic* or *Magnecrystalline*, etc. (9703).

9707. *Bismuth*. Have separated a cube from a crystalline mass.

It is not complete, for some of the faces are such as have been formed on purpose and finished on a stone, and there is also a large cleavage association of planes in one direction. By means of a cork model, I have taken note of the various faces, and so can by them refer to either the angles or the plane. It was first suspended by four of the solid angles, and as the planes were numbered 1, 2, 3, 4, 5, 6, these angles were taken round plane 1—and then it was suspended by the centres of three of the planes, being those which were at right angles to each other. The results were as follows, at the Electro and also at the common horseshoe magnet.

9708. Suspended by the angle of planes 1, 2, 3, it stood thus, looking down upon it from above.

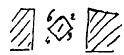


9709. From solid angle 1, 3, 4—then stood thus*.

9710. From angle 1, 4, 6—thus†; and nearly indifferent as to set.

9711. From angle 1, 6, 2—thus‡.

9712. When suspended from plane 1, and therefore that and the correspondant plane horizontal, it stood thus. Suspended from plane 2, thus§; and when suspended from plane 3, thus||.

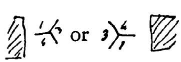


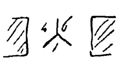
9713. Now it was examined for the axial Magnecrystallic line by hanging it in two positions, and the line was found to be through the crystal or cube from angle 1, 4, 6 to angle 2, 3, 5; the latter angle was found replaced by a small cleavage plane, which was bright, and evidently the cleavage plane which points to the poles.

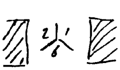
9714. For examining the above positions, this plane will be found always directed to one of the poles—and now when suspended with that plane vertical, it pointed diametrally towards the magnetic poles, and when placed horizontally, the cube scarcely pointed at all. Further, by inclining the crystal so that this plane, though nearly horizontal, should still incline one way or the other, the crystal could be made to point any way I liked, for the plane perpendicular to this facet was always in the axial line.

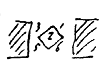
18 SEPTR. 1848. (WIMBLEDON).

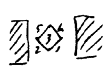
9715. The native *copper* crystal (9693) was hung apex upwards between the poles of the horseshoe magnet. If rotating, it was instantly stopped when between the poles. The torsion being taken off, the crystal did not set in any particular way and obeyed

* [9709] 

† [9710] 

‡ [9711] 

§ [9712] 

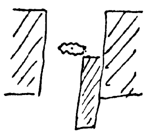
|| [9712] 

the slightest torsion put on above, though slowly only. The length was then made horizontal; still the effects were the same, and in no position was there any signs of Magnecrystallic force or condition.

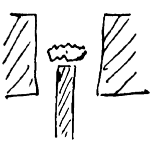
9716. Another crystal of *copper* gave the same result.

9717. The crystal of *lead* (9698) hung with the length horizontal; it rotates freely in the magnetic field, as if it did not feel this magnetic force. It sets with the apex to one of the poles, but did not set diametrally, and the set was found due to torsion, for any degree of torsion given above was instantly obeyed below. It changes its position with torsion so freely as to be in striking contrast with most of the metals heretofore experimented with—just as in the free air indeed. No signs of Magnecrystallic force here.

9718. The *Bismuth* piece No. 1 was hung up in the Magnetic field with the Magnecrystallic axis horizontal—and set of course well. When quiescent, a piece of soft iron was brought near it, and affected it. If the iron was in contact with the magnet and also near one end of the bismuth at the side, that end approached as if attracted by the iron—or if the iron were separated a little way from the magnet, but still virtually formed a prolongation of its cheek, it acted in the same way, as if attracting the bismuth. This happened on either side of the magnet and therefore on either end of the bismuth, and either side of the piece. The iron virtually is a prolongation of the magnet, and the magnetic curves are altered and diverted in their course, and now the most powerful lines of force proceed from the part of the iron nearest the affected end of the bismuth. The results are, I think, as they ought to be.



9719. The iron being removed and the bismuth left, if the pole of a separate magnet were approached in a horizontal direction midway towards the bismuth, then it was much affected. If the pole were a north pole, then the end of the bismuth towards the north pole of the horseshoe magnet approached this extra pole as if attracted by it, and the other end receeded as if repelled. If the pole approached were a south pole, the contrary effect was produced. A small magnet is sufficient for this purpose. I used the end of the blade of my pocket knife, and even a needle can affect the bismuth sensibly.

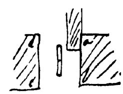


9720. This again, I think, is quite consistent, and though the effects appear like attraction and repulsion due to ordinary magnetic forces, can just as well be account[ed] for by the tendency of the Magnecrystalline force to set in a certain position in relation to the magnetic lines of force, and on other account the idea of attractive forces under the circumstances is not probable.

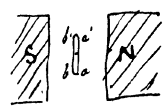
9721. Use a plate here, and then shall be able to procure a form of the experiment in which the parts of the mass shall receed.

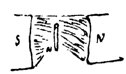
19 SEPTR. 1848.

9722. Hung a plate of Bismuth between the cheeks of the horseshoe magnet; it obeyed and pointed very well, with the faces towards the magnetic poles. Then a piece of soft iron between the magnet cheek and either end or side of the plate caused the plate to move from it, as if it repelled it. Thus if at *a*, it sent off the edge of the plate nearest to it—or if at *b*, *c* or *d*, it did the same thing. If the whole space from *a* to *b* were filled up with iron, the plate did not move, i.e. it was not bodily repelled. This is all quite consistent, not with any real repulsion, but with the law that the plate *b*, as a plate, would tend to set across the lines of magnetic force, the Magnecrystallic axis tending to place itself parallel to or as a tangent to them (9730).



9723. Took a small Magnetic pole (the end of my knife); it was an N pole, and being held at *a* or *a′*, i.e. on the side of the plate towards the N pole of the electromagnet, it moved the plate as if it were repelled, and just as the piece of Iron did. This should of course be so. When held at *b*, *b′*, it seemed rather to attract the plate or cause its motion towards it, the pole—and there is no doubt but that a N Pole there would cause the resultant of magnetic force passing through the plate to be oblique, and therefore tend to set round the plate in that direction. When the pole of the knife was held at *c*, I could not perceive that it affected the bismuth plate sensibly with this magnet. But I must repeat the results with the electro magnet and a good hard steel magnet small pole.





9724. If the approximated pole were an S pole, then all these actions were reversed, being as it were repeated on the contrary side of the bismuth plate.

9725. When the bismuth plate was raised above the level of the poles and therefore out from between the cheeks, the motions were modified and not so strong.

9726. The apparently contrary motions (9718, 9722) and also the others (9719, 23, etc.) are all in harmony with the law of setting action which I have assumed.

9727. Magnetism may supply the best means of distinguishing the essential and real form of bismuth, antimony, etc. better than crystallography or light.

* [The diagrams at the foot are drawn on a separate slip which is bound into the MS. at this point.]

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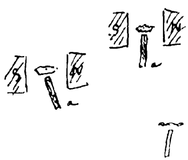
9728. A crystal or plate of bismuth should point in a helix or ring carrying a current.

9729. Is the Electro deposition of metals affected or modified in the intense electro magnetic field?

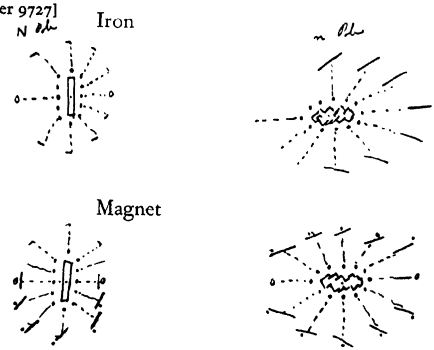
9730. *Crystallized bismuth* and *Iron* action in the Magnetic field. The horseshoe magnet was used (9539). The crystal of bismuth was No. 1 (9540) as before, and when the same piece of iron was applied as on the former occasion, the results were exactly the same (9722) in nature.



9731. When the piece of iron was parallel to and in the equatorial line thus, it did not affect the position of the crystal; but if the end *a* were inclined to either side, [it did], and the more as the iron was more inclined, until the *a* part touched the magnetic pole, when the effect was a maximum.

9732. The effect was of the following nature: the crystal being at first perpendicular to the soft iron, as the iron was inclined, it moved in the contrary direction, as if tending to place itself

* [After 9727]



parallel to it, and was the most deflected from its first or axial position when the iron touched the pole by its end *a**.

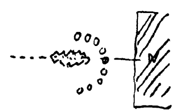
 or 

9733. It is manifest that when the iron was at first in the equatorial line, its end near the crystal was not a pole, though it might have common magnetic polarity on its two sides; but that as the end *a* was carried to one side or the other, the piece would become polar in its length, and the more so as the end *a* was nearer to the magnet towards which it was approaching. When therefore, as in the figure, that end touched the N Pole, the end of the piece near the middle part of the crystal would also be an intense small N pole, and when the termination *a* touched or was near the S pole, the end at the middle of the magnet would be an S pole.

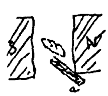
9734. This explains the whole effect, for when, as in the figure, *a* touched N, the lines of Magnetic force would not be equal between S and N, but would converge intensely upon the other end of the soft iron, and the motion of the crystal was a simple result of its endeavour to place itself, or rather its Magnecrystallic axis, parallel to or as a tangent to the resultant of those passing through it. The same would be the case if the soft iron were carried towards S at its outer end *a*, or if it were applied in a similar way on the further side of the magnetic field.

9735. A small rod of iron about 0·2 of an inch in diameter shews the results more clearly than a thicker one, and if rounded or bluntly pointed at the end near the bismuth, is better still, and for manifest reasons, for the new pole formed is more distinct and intense and the lines of force proceeding from it more curved in their direction.

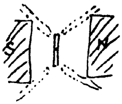
9736. When the piece of iron, having the end *a* against one cheek of the magnet, had the other (constituting the moveable subsidiary pole) between the magnet and the crystal, as marked by the dots ◍, then it did not affect its position; for though the lines of force would converge on the end of the iron, yet the resultant of those going through the crystal would be in the same position as before; but when it was moved to the right or left of the axial line, then the crystal moved with it: not so much as it generally did, but at times I have even been able to pull the crystal round, because of the intensity of action close to this moving subsidiary pole. It is easily seen why this happens, for the moving end is as



* [9732]

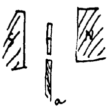


a conical pole applied close to the crystal, and the law makes the crystal tend at all times to place its Magnecrystallic axis parallel or as a tangent to the magnetic curves issuing from it.



9737. The *bismuth plate* No. 8 (9550) was now put into the Magnetic field. When the soft iron rod was applied in any of the positions represented, it seemed to repel the plate, simply because in any of these positions the plate turned so as to place its magnecrystallic axis parallel or as a tangent to the resultant of the many magnetic lines of force passing through, and *must* move as represented* to do so.

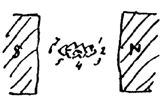
9738. If the iron were brought over from the other side, the plate moved as represented—still a result of the same law, for it is quite clear that the part of the crystal plate near the iron has not curves of such force or direction passing through it as before: that the lines passing through its middle and more distant parts must be now oblique, and in that direction which agrees with and requires the altered position of the plate.



9739. When the iron was equatorial and at the edge of the plate, it did not alter its position sensibly.

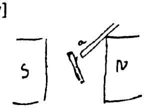
9740. A plate of *Arsenic* presented the same phenomena. When the travelling end of the soft iron was carried between it and the cheek of the magnet (9736), the result was very manifest and explanatory.

9741. The general experiments were now repeated, using instead of the *soft iron*, the pole of a *permanent magnet*. In these experiments, the approached pole should not be too large, as then its action is more diffuse and not so powerful, because it cannot be so closely approached to the required part of the bismuth. A pair of pliers magnetised gave too large a pole—a needle gave too small and weak a pole. The end of the blade of my pocket knife acted excellently well.



9742. Bismuth crystal No. 1 (9540) was suspended in the Magnetic field and an N pole approached. At 1, 2, 3 it acted just as iron touching the N pole would do (9733, 9736) and for the same reason. At 4, though equatorial, it acted as iron touching the N pole, and properly so, for as the iron becomes such a pole by position, this is so in itself. At 5 the bismuth receeded from it—at 6 the action was *nil.* At 7 the bismuth receeded from it in the contrary direction.

* [9737]



9743. All this is as it ought to be, for the action being nil at 2 and 6, the diametral positions, all the way from 2 by 3, 4 and 5 up to 6 its tendency is to set the axis of the crystal round one way, as if it would follow the motion of this supplementary magnetic pole. In fact, the apex of the N cone of lines of force which it sustained travelled thus, and the crystal tended continually to place its magnecrystallic axis parallel or as a tangent to the resultant of the changing magnetic curves passing through it.

9744. An approximated S pole did exactly the same thing in the opposite direction.

9745*. The *bismuth plate* No. 8 (9550) was now hung up and subjected to the action of the *extra Magnetic pole*. An N pole at 1 indifferent; at 2, 3, 4, set the plate round thus; at 5, indifferent; at 6, 7, 8, set the plate round thus. The S pole produced the reverse actions in the contrary direction. So the apparent repulsion in positions between 1 and 3 or 1 and 7, the tangential action at 3 and 7 and the apparent attraction between 3 and 5, and 7 and 5, all resolve themselves into the endeavour of the plate to place its Magnecrystallic axis in the corresponding right position, as the Magnetic curves passing thrgh. it are altered in force and direction by the travelling of the extra north pole from 1 round to 5 in either direction. The results with the south pole are the same in the opposite direction.

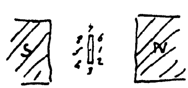
9746. The *Plate of Antimony* No. 12 (9631) presented the same results.

9747. The *Plate of Arsenic* No.   (9694) gave the same results and very well indeed.

9748. *Bismuth crystal in Water*. A vessel of water was placed between the cheeks of the horseshoe magnet (9539), and the bismuth crystal No. 1 (9540) hung in it. The crystal pointed well; there was even a vibration or two, but slow and soon over. When displaced, it returned to its true axial position as in air. When the water and all was still, and no torsion on the suspending fibre, it required five revolutions of the index above to produce torsion force sufficient to carry the crystal away and turn it round.

9749. Saturated solution of proto sulphate of Iron was employed in place of water. Here also the crystal of bismuth pointed well and apparently as before. Here also it required five revolutions

* [9745]



of the torsion index to carry it round. So that the force of set, or the force which gives it position, appears to be the same in amount whether the crystal be in air, or water, or solution of Sulphate of iron, or between thick masses of bismuth (9702).

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9750. *Antimony*. The revulsive plates No. 22 (9627). Took one of them, now to be call[ed] No. 30, and examined it at the horse shoe magnet. There were no signs of arrest; the magnet. force is not sufficient for this, neither can it be put on suddenly so as to make its effect evident by comparison—but the plate vibrated. On examining it, found that the large planes were not those which faced the magnetic poles: the Magnecrystallic axis was obliquely across the plate. When suspended horizontally, therefore, it also pointed, and it ought also to point in this position at the Electro magnet.

9751. When suspended by the edge, it pointed with but feeble force, one revolution of the torsion index overcoming it. When suspended horizontally, the force was greater, $1\frac{1}{2}$ revolutions being now required. The plate is probably a mixed plate: and the planes (large) are evidently not perpendicular to the *Magnecrystallic* axis.

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9752. The same plate was taken the next day and examined carefully at the great Electro magnet ( ). When suspended at the edges, in two places, it does point and in the same direction as at the horseshoe magnet—only the plate is arrested and afterwards goes slowly up to its position ( ). If whilst in an oblique position the current be broken, revulsion takes place; and this may happen whether the mass has gone up to its pointing position or not, for its pointing position is oblique, and the oblique position is the revulsive position. If the magnetic force be continued, the plate always goes at last into the same position, which it acquires and vibrates about at the horseshoe magnet.

9753. Hence a common magnet is even better than an intense electro magnet, because, not producing the inductive arrest effect

to any serious degree, it does not interfere so much with the magnecrystallic effect.

9754. Does this indicate that the Magnecrystallic force is an original force?—or else that it is more easily induced than the currents?—or else that it comes to a maximum sooner?—or what?

9755. When this plate No. 30 was horizontal, it was not arrested or revulsed, there being no sufficient extension in the vertical direction, but it pointed, and just to the same position as with the horse shoe magnet.

9756. Another *revulsive Antimony plate* was taken from packet No. 22 (9627) and now called No. 31. On the 26th (yesterday), it was examined at the horse shoe magnet. Hung up by the end, it set obliquely across the Magnetic field, and also *diametrally*: and being suspended again vertically, it was found to point by a line going obliquely through its length. Still, when hung horizontally (i.e. planes horizontal) it gave another line. There was no indication of arrest and revulsion here. Now will this piece, by the great Electro-Magnet, shew only revulsive power? Or will the pointing come out? And if broken up, will the revulsion cease? The pointing is not by line of power perpendicular to the planes and therefore cannot expect pointing to come on by breaking up the plate ( ).

9757. To-day[1], the plate was tried by the great magnet (9756). When suspended by the end, it set as at the horseshoe magnet—only it went up slowly to its position—it is arrested or retarded and revulsed at making and breaking contact. When hung by the edge with the length nearly horizontal, the set was just as at the horse shoe magnet, but going up to the position slowly. When the planes were horizontal, it set as, but better, i.e. quicker and stronger than, at the horse shoe magnet: but there is no arrest, retardation or revulsion now. All right and clear now (9626).

9758. I wished to place this plate with gum or cement ( ) between paper or mica—or tin foil, or some other flexible thing, which when the piece was bruised, could hold the pieces in their places. But all paper was magnetic. Mica was either magnetic

[1] The date 27 Aug. 1848 is written over this paragraph in the MS, evidently in error for 27 Septr.

or Magnecrystallic, and copper or tin foil shewed of themselves the arrest and revulsive action strongly. I used at last a paper selected as being least magnetic, and enclosed the plate of bismuth between 2 folds of it. Then I hung it up in the Magnetic field and noticed the arrest and revulsive effect, after which, pressing on it in a mortar with a pestel, I broke it into many pieces which still held together in their original place and form, and now it was much less affected as to arrest and revulsion than before.

9759. Antimony No. 21 (9624), being again submitted to the Electro magnet, was found as before to point, freely vibrating, and not to shew signs of arrest or revulse: but it is a thick narrow plate. It seems well crystallized, and so has much magnecrystallic force, but its breadth is not sufficient for the generation of sensible inductive currents, and its comparative thickness again tends to diminish such currents.

9760. Hung up Bismuth No. 1 (9540). Arranged a helix near it, so that the crystal was in the axis at the mouth of the helix, and then sent the current of 10 pr. of Grove's plates through the helix. Immediately the crystal pointed and could point diametrally. The magnecrystallic axis was in the axis of the helix—and the crystal vibrated well, I think with about half the force it might do at the horse shoe magnet.

9761. So there are hopes that a crystal can induce a current, but the force will be very small. The question is, how is the crystal to be moved. Now, as a magnet, if carried quite through a helix, produces no final result, because the two poles are contrary to each other, here, as they are the same, perhaps the passage of the crystal *quite through* one way may produce one current, and back again, the other.

9762. *Zinc*. Tried some small fragments of zinc. They pointed strongly but I think from iron; examine matters more carefully. The zinc shewed signs of arrest and revulsion.

9763. Dip is about 70° or 71°.

9764. *Small horse shoe magnet*—its weight is $7\frac{3}{4}$ oz.—or 3650 gr., and it can raise at the keeper $22\frac{1}{2}$ oz.

9765. It is easy to observe the revulsive action in a crystal that does point magnecrystallically if by a stop the crystal be kept in an oblique position, the stop being applied on that side which does not interfere when the revulsive action is brought on.

9766. As to easy production of the phenomena by bismuth. With the crystal (9699) and the small magnet (9764) obtained them readily. The small magnet was placed horizontally, the chops are about $\frac{1}{3}$ of an inch apart, and the crystal, either between them or before them, was well affected.

9767. Even when the end of the pliers and of my pocket knife (   ) were used as the two poles, the crystal obeyed and vibrated; though neither one could support the other, and the knife did not weigh much above an ounce.

9768. *Zinc.* Tried several pieces, and though there were signs of Magnecrystallic power, yet the phenomena are too confused to allow of any trust or confidence. A piece of the zinc would point perhaps lengthways, perhaps obliquely—and require 2 or 3 torsion turns of the index to move it away; but it would not set well diametrally, if at all, but seemed, by standing in one position in the magnetic field, to have acquired a polarity which would probably be magnetic, and due to iron—for iron must be in this ordinary zinc.

9769. Does the filament of silk preserve all its elasticity, or does it take a set in 6, 8, or more turns?

9770. Put a wire to its end, and (without magnet) stopping the end, applied 6 turns to the index above. After 2 or 3 minutes, let it down, allowing one revolution of the wire at a time; it only went thrgh. $5\frac{1}{2}$ before it came to rest. Again, put on 10 turns and after a few minutes let it down—it only passed thrgh. $9\frac{1}{2}$ revolutions to come to rest—being left a while, it seemed to recover a part of its loss.

9771. The filament is not compound. Perhaps a bifilar suspension more unexceptionable for coarser experiments.

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9772. What would ordinary static electric induction do with a globe of regularly crystallized bismuth? I think nothing, for if a conductor, after that internal condition has nothing to do with static induction; it is the form of the outside and its conducting ability that is concerned.

9773. Placed a crystallized piece of bismuth (   ), with its *magnecrystallic axis* horizontal, in the middle of the circle described

by the magnet *i* of the apparatus described (9432) and level with its lower pole. It certainly affects the magnet and makes it move very distinctly, changing its final place of rest as the bismuth is turned into another position; but I have not yet made out the places of rest in relation to the magnetic axis. The observations are long and I must leave them until I return from Wimbledon.

9774. Remember that a small quantity of ferruginous matter and its magnetic effect comes out first, and may appear here in a piece of bismuth subject to a very small magnet, which if subjected to a much more powerful magnet, might have that effect overpowered and hid by the increased magnecrystallic effect.

9775. But then that would indicate that the magnecrystallic effect was an effect of induction almost certainly. Can I not in this way decide what is original and what induced?

9776. If it be an induction, then it is almost certain that Plückner's results and mine are due generally to the same cause, i.e. crystalline structure.

9777. In trying mutual action of crystals, put one under another, concentric with it and with magnecrystallic axes horizontal—this is the position in which they ought to affect each other.

9778. Do the same in the magnetic field, the line of magnetic force being vertical, and move the under to see if it will affect the upper and suspended one.

3 OCTR. 1848. (WIMBLEDON).

9779. Examined crystals of Sul. Iron and Sul. Nickel as magnetic salts for any facility of giving Magnecrystallic phenomena, which perhaps they have above salts which are not of Magnetic metals. Used the horse shoe magnet (   ) and obtained plenty of good evidence.

9780. *Sulphate of Iron* (*proto*). The crystal not affected except magnetically, and by length going axially; reduced its size gradually until the dimensions in the horizontal direction were as nearly as might be the same, and still could get no distinct effects. All appeared to be magnetic.

9781. But now moved the point of suspension 90°, and instantly the piece affected strongly, and the shortest dimension was axial. Removed it again 90°, so that still shortest dimension could go

axial, and it still went so. So that the Crystal is Magnecrystallic—by axial power. Soon found that the axial line was perpendicular to 2 given planes of the crystal, and then could and did take 8 or 9 rough crystals and suspend them one after another with the anticipated line of force horizontal. It always went axial with force, and yet in all these cases the greatest length was not in that direction. It was often 2 and 3 times the length of the axial dimension, and yet it went equatorial, or into any oblique position, so that the Magnecrystallic axis was axial.

9782. Give the direction of this axis and of the optic axis in the crystal after examng. them at home. Also try these crystals by the Electro magnet. I expect that, as in the tourmaline, Red ferro prussiate, etc., strong power will develope the Magnetic force and make them point (9893).

Sulphate of Nickel (9894).

9783. First tried two large crystals, but they seemed to set always with the greatest horizontal dimension axially, as if by magnetism.

9784. Then set to, more carefully, to examine a piece of nearly equal dimensions. Have a crystal of this form and size*. Cut a piece out of the middle as shewn by the dotted lines, but to identify the direction of forces, will always call the direction from *a* to *b* the length, from *c* to *d* the breadth, and from *e* to *f* the thickness of the piece, whatever its real dimensions may be reduced to.

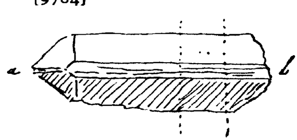
9785. Went on reducing this piece by water grinding, and trying it continually, until at last it was a parallellopipedon of which the breadth i was the largest dimension—the thickness ii a little less and the length iii considerably the least. At first the crystal was partly covered with a crust of other crystals, perhaps sulphate of nickel and potassa, harder to dissolve than the sulphate of nickel, but these had been all removed and the piece was a beautiful clear green substance, very pellucid within, and apparently very regular in crystallization.

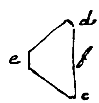
9786. Had many indications of Magnecrystallic action, but the following sums up the whole.

9787. Suspended it with the *thickness* ii *vertical*—it pointed feebly and with the breadth i, being the greatest horizontal dimension, oblique axially—as if the piece set chiefly by Magnetic force.

9788. Suspended it with the length iii vertical. It now pointed

* [9784]





well and with the breadth i oblique axially as before; the breadth was still the largest horizontal dimension but the equatorial dimension, being the thickness ii, was greater than before. So that as to magnetic set, it ought to have been weaker than before, whereas the set was really stronger, as if some other influence helped it. The thickness is equatorial; can it have any optic axis effect?

9789. Suspended it with the breadth i vertical. Now the pointing was more powerful than in any other position, and the length iii went axially, being the least dimension, and the thickness equatorial. Here the effect cannot be magnetic, but either Optic axis or Magnecrystallic or both.

9790. Considering the two last suspensions, I think it looks as if there were both Optic axis and Magnecrystallic force—or as Plückner would perhaps say, 2 optic axes. Look for optic axes—and relate them to the crystal—also try them at Electromagnet and define in what direction the Mag. crystallic axis lies in the crystal.

9791. A Magnecrystallic crystal may have two axes of force and, if the power chiefly evolved by induction, that in which induction prevails at the time be then the stronger though in comparison the weaker when compared with the other—in such a case there might either be two directions of set or position, or the weaker might be a case of instable equilibrium and so always give way to the stronger.

9792. In such case, the weaker would add its influence and so produce results resembling the optic axis results of Plücker.

9793. *Sul. Nickel.* Consider it as having two Magnecrystallic axes at angles with each other—either equal in force or one stronger than the other. The latter like to be the case.

9794. Consider red ferro prussiate and learn that it has really 2 optic axes.

9795. Try tourmaline as to mutual action.

9796. Remarkable that it should be the Magnetic metals which give chiefly the crystals shewing Magnecrystallic properties. Can calcium be a magnetic body?

9797. Have obtained various crystals from Mr Tennant and now record the results obtained with the horseshoe magnet (9449), with the poles approximated by putting a cube of iron between, or opened out by removing it.

9798. *Diamond*—a clear, colourless, well crystallized Octoedron. Suspended at angles and faces. No signs of any Magnecrystallic phenomena (9864).

9799. *Gold*—a good solid independant crystal with pentagonal faces I think, but somewhat rounded or worn. It shewed no signs of Magnecrystallic phenomena, but was evidently retarded, and at the E. Magnet would, I have no doubt, shew the arrest and revulsive action (9867).

9800. *Boracite*. Two excellent clear crystals—free from Magnetism. Gave no signs of Magnecrystallic force (9885).

9801. *Oxide of Titanium*. Brookite. A flat crystal piece—thickness about 0·1 of inch—breadth and length irregular, but about 0·4 or 0·5 of inch. Suspended in any way, the greatest horizontal dimension went axially, but very weakly, as if the whole were a little magnetic; but there were no signs of Magnecrystallic phenomena (9876).

9802. *Red Oxide of Copper*. Good crystals as to form but covered with Carbonate copper, and seem to be a little Magnetic. No clear signs of Magnecrystallic action—outside too impure.

9803. *Oxide of Tin*. Small square prism about 0·5 of inch long, 0·2 broad and 0·1 thick. One end crystal facets—the other broken. Being suspended in all directions there were no signs of Magnecrystallic action. No magnetism. Good crystal (9869).

9804. A larger crystal of the usual form, with little mica gangue adhering—gave a little pointing power but that was in the gangue (magnetic) and not in the crystal.

9805. *Red Silver*. A fine but compound crystal, but the chief one far larger than the others. With the prism (and length) vertical, there was no set or pointing—with the prism horizontal in any direction it pointed a little axially. There is a little pointing power but whether Magnetic or Magnecrystallic cannot say just now: is very small (9886).

9806. *Magnetic oxide of iron*. Octoedron. Will set in any long direction—is too magnetic altogether.

9807. *Specular iron.* Good crystals—but too magnetic—the longest dimension (horizontal) always strongly axial.

9808. *Iron pyrites*—a fine yellow cube—perfectly indifferent Magnecrystallically—only a trace magnetic (9887).

9809. *Arsenical Iron.* Three crystals. These looked very promising at first, and the crystal first tried gave peculiar and regular set—then they became confused and then the other crystals set differently. At last changed the cement and lost many signs. On the whole, there are appearances which make me think that unexceptionable crystals would give some Magnecrystallic results—but these appear to be too irregular in composition (9888).

9810. *Copper pyrites.* A tetrahedron, having one solid angle deeply truncated so as to give a triangular thick plate. There were curious and oblique sets of this plate and I think there are Magnecrystallic appearances, but it would require better crystals and comparison of several with each other.

9811. *Bright white cobalt*—a fine octoedral crystal. Points (when all cared for) uncertainly and feebly—must try the Electro magnet (9889).

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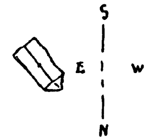
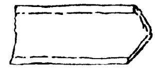
9812. *Mutual action of crystals.* Red ferro pruss. potash. The plane passing through the obtuse linear angles of the prism is that in which the optic axes (according to Plücker) lie; so took a large crystal and ground off one of these angles, so as to expose a large face, and fixed the crystal on a cork as a stand, with this face upwards. A smaller crystal had also a similar flat face produced, and it was then hung up by a single cocoon filament with the face downwds., and at such a height that when brought over the other, the new faces were towards each other and only $\frac{1}{20}$ of an inch apart; so that the upper could move over the first, and if it was natural, place itself over it and symmetrical, as regarded crystalline structure. The whole was covered with a glass jar—and that so moved that the one was away from the other 2 inches or more. As something may depend upon it, I will record their relative positions in a diagram, and in moving the crystals, and looking at the diagrams, the top of the page may be taken for

the South, for I was working at a table in a window looking directly South.

9813. When the small crystal was at rest, it stood about thus in plan, as compared to the larger one which was fixed; but being then by the motion of the glass jar brought over the larger and left, it moved and finally took up a position parallel to it*: and I think I saw that, as it went up to the position, it was accellerated in its motion, and as it passed, in vibrating beyond it, it was retarded. Seems Good.



9814. When stationary, I turned the torsion index round 90°, which only made the little crystal incline slightly —. With 180° it vibrated a little and stood thus —. With 270° it moved more —, but did *not move off*. With 360° did not get away. With 450° of torsion, it left and swung more than round, and then vibrated about a little irregularly, as if moved a little by currents in the Jar. The jar was glass, about 12 inches high and nearly 8 inches in diameter. The upper crystal tended to settle parallel to the large one as before.



9815. Must make the experiments in a place of equal and equable temperature, as the froggery. Must be aware of any possible electrical effects, though these not likely here—warmth of the hand applied to the jar sides in moving it.

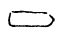
9816. The lower crystal may form a block to the currents in the jar and so the latter get into position near it, as in an eddy—or may be influenced by the diverted streams of air. Can apply sides to the lower crystal, so as to shut off these currents.

9817. Or can make the lower crystal a chamber built up of crystals symmetrically placed and surrounding the moving one on all sides—so both influencing and sheltering it.

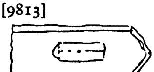
9818. The upper crystal being removed away from the lower, continued slowly to move a little, as if by currents. The window I am working at is a warm window and the sun is shining on the holland sun blind.

9819. There is, I think, no doubt the crystals affect each other; the Essential point is—the cause?

9820. Turned the lower crystal a quarter round so as to stand with its length *North and South*. The other crystal, being thus and still, was then brought over it; it moved, but more than



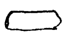
* [9813]



the crystalline influence alone would produce, and went 270° or more round. It seemed inclined to set thus, oblique across it, but the vibrations are not free and the motions are continually changing in direction. However, if there be any crystalline power exerted, I think it must be weak only, under the circumstances.

9821. After a time, the small crystal was right across the other, as if there were currents or influences in the jar that kept it there. The crystals, when one over the other, are in the *middle of the jar*. After an hour, still it was in the same position and not moved, except perhaps a few degrees to and fro. So that whatever position the under one has–this seems to have a constant position. Being moved away from the lower and left, it still retained the E and W position. After half an hour, being moved over the lower one, it remained *unaffected by it*.

9822. Now put on torsion force, and after 360° were given, the crystal had not gone round–it only stood a little E by S and W by N. So there must be something acting on the crystals that keeps it so. I warmed the side of the jar by the hand, but that did not disturb the crystal as much as I expected the currents would have done.



9823. Being away from the lower crystal, it was left two hours or more and found still standing thus, E and W. Left handed torsion was then put on; 180° made the position only thus; a torsion up to 270° caused it to go round and even to go on to 360° and rest nearly there.

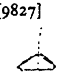
9824. I cannot contrive by putting on torsion to bring the crystal into N and S; it always swings away from and keeps beyond it.

9825. Before trying experiments of mutual action, the first thing is to see that a single suspended crystal will stand in any position in a jar. It puts one in mind of Baily's troubles with his earth weighing apparatus.

9826. Can the effect be a result of the tendency which the optic axis plane has to set itself across the line of the dip? It may be so. If so, then inclining this plane so as to form an angle of 70° with the horizon ought to give it more E and W directive force.

9827. Suspended the crystal so that, looking end on, it did not hang thus* but thus†.

* [9827]



†

9828. Now being left to itself and the under crystal away, it stood nearly E and W–but with the plane of the Optic axis coinciding with the dip and not as oblique as it could be to it. Put on 90° of torsion–the crystal went round a little–made the torsion 180° and now the crystal went round 360° or more, and settled as before (at 360°), east and west with Optic plane again parallel to the dip.

9829. The point of suspension was now slipped over the ridge or linear angle of the prism, so that the inclination of the optic axis plane was on the other side of a perpendicular, and reversed as regarded the pyramid end of the crystal, which pointed W in the last experiments.

9830. The crystal now went round and rested about thus, so that pyramid termination was towards the east; but still the optic plane tended to parallelism with the mag. dip. There was no difference in the torsion here.

9831. Is it possible that the Sun's rays or the light can affect the setting of the crystal? The rays were not directly on the glass jar but the sun shone on the blind, and the crystal always tended to place itself parallel to the window. Try a crystal in the sun's light (9854).

9832. The Sun is now going off and the crystal, whether from light, warmth or what, is more uncertain. Opened the window and sent the Sun's rays (½ p. 4 o'clk. Octr. 6) by a piece of *plain glass* on to the crystal. It moved much, but whether by the light or the cool draft of air against the jar, I cannot tell at present.

9TH OCTR. 1848. (LONDON) *home altogether now.*

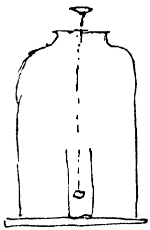
9833. This morning I mounted 12 specimens (suspending them by single cocoon threads) in glass jars, and put them on a steady table on the stairs from the Laboratory to the library (which are of stone and dark), to see if they were influenced by the Earth. The silk was in general about 9 inches long and some very delicate and therefore having very little torsion.

No. 1 Crystald. Bismuth–its *Mag. crystallic axis* marked in notes thus ——

No. 2 Do. . . . .

No. 3 Do. . . . .

No. 4 Crystal. Sul. Iron . . its *Mag. crystallic axis* marked in notes thus——
No. 5 Tourmaline . . . . its Optic axis . . . Do. .
No. 6 Do. . . . .
No. 7 Crystal. Red ferro pruss.—its Optic plane . . . . .
No. 8 Do. . . . .
No. 9 Do. . . . .
No. 10 Do. . . . .
No. 11 Do. . . . .
No. 12 Do. . . . .



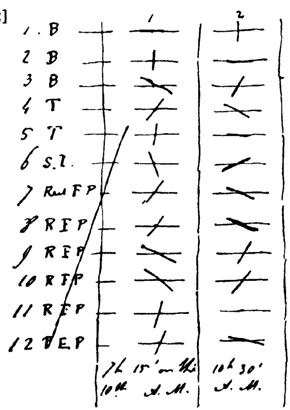
The magnecrystallic axis and the optic axis were horizontal; the optic plane of the 7–12 were vertical.

9834. The crystals continued to move slowly all the day, and as I believed this to be from currents of air in the Jars, I introduced into each Jar a flask or glass, or mercurial jar or other sheltering vessel, that could hinder the general currents in the great jar; in 1½ hour after, all were steady and free from movement or nearly so—left an indicator by each jar to shew how axis, etc. stood.

9835. Next day, 10th, at 7 A.M., all the crystal[s] except 4 and 6 were as left; they had not moved. Adjusted the indicators of 4 and 6 and left them awhile; they were as follows*, the horizontal line being parallel to the front of the table and the long line the direction of Mag. Needle.

9836. Then at 7h. 15′ I turned the torsion index 90° to the right in all and left them—at 10h. 30′, or 3 hours 15′ afterwards, I found the crystals all in their new positions and quite still, except No. 11, which I think had touched the inner jar. Now all the indicators required turning right handed thrgh. 90° to correspond with the

* [9835]



crystals. This was done and column 2 of the positions shews the state of things now.

9837. Then at 10h. 30′ put on 45° torsion right handed—at 1h. 45′ P.M., or in 3h. 15′—found all had moved, and come to rest 45° in advance of their last position. Rectified the indicators, and again put on 45° of torsion. At 4h. 0′ P.M.—all advanced 45°—adjusted the indicators and then put the torsion handles back 90°.

9838. Wednesday, 11th Octr. at 9h. 30′ P.M., had to move indicators of all back 90° to coincide with the positions of the crystals. Put the torsion hands back again 45°.

9839. Thursday, 12th, at 7h. 30′ A.M., the indicators all required moved backwds. 45°.

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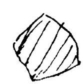
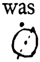
9840. So it is manifest that there is no sensible action of the earth in any of these cases—for the 12 specimens are in very various positions and any or all of them may have any degree of torsion force put upon the suspender in any direction, and still the crystal follows and moves just as many degrees. Now as this happens in every part of the circle of revolution, it is manifest that there is no holding power in the direction of the Magnecrystallic axis or in any other direction.

9841. No doubt the position in this latitude is not so favourable as it would be at the equator—and even at the equator there is no reason to expect any great degree of action—perhaps it might be there insensible. For in none of these cases do the lines of terrestrial magnetic force equal the lines of force given by my knife (9767) or those in the suspended magnet arrangement (9433).

9842. Experimented with hung Magnet (9433) and a group of crystals of bismuth on the support, the Magnecrystallic axis being horizontal. In making notes, I will represent things thus, where the circle *d* represents the piece of bismuth (which had an irregular shape in reality), the point *a* the center upon which it revolved, and also the center of motion of the slung magnet *c*—*c* the place of the lower pole, which was below the level of the piece of bismuth, and *b* the direction of the Magnecrystallic axis of the bismuth and one of the ends of that axis as distinguished from the other.

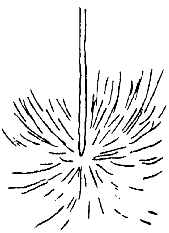


9843. On *Octr 7th*, at 12h. 0, all was in position and left to rest. At 7h. 30′ P.M., things were thus  :—the bismuth was moved

into this position and at 10h. 15′ P.M. the positions were —i.e. the Mag. pole apparently unaffected. The bismuth was made —then

9844. *Octr. 8th*, at 8h. 0′ A.M., the Mag. Pole was as before—but because of the shape of the bismuth, it and the Magnetic pole were a good way apart: the bismuth was advanced :—at 9h. 0′ P.M. the M. Pole was or nearly as before—put the bismuth on to .

9845. *Octr. 9th*, 6h. 30′ A.M., the positions were : the Magnetic pole had moved to the right distinctly as if it had tried to follow the Magnecrystallic axis of the bismuth. Moved the bismuth on to . Then at 12h. 0′ or midday, was : the Magnetic pole had gone still more to the right, as if trying to leave the equatorial line and follow the axial line. There is no doubt that the Mag. pole is affected by the bismuth, but whether *Magnetically* or *Magnecrystallically* I cannot tell yet. Advanced the bismuth to :—at 4h. 0′ P.M. things were —the Magnetic pole had returned to the left and approximated to the other end of the Magnecrystallic axis.



9846. Up to this time, the level of the bismuth was almost $\frac{3}{4}$ of an inch above the level of the lower end of the needle magnet, and as the lines of Mag. force there would hardly be horizontal, but very oblique upwards, I now lowered the bismuth so as to place it level with the lower end of the magnet.

9847. So at 4h. 0′ P.M. Matters were left thus . At 5h. 30′ P.M. the M. Pole had gone to the right as if driven from the Mag. crystallic axis, or as if drawn by some ordinary magnetic influence. I therefore removed the bismuth away from the Magnet and left the latter to take up its own position due to the torsion of the suspending filaments.

9848. At 6h. 30′ P.M. The needle pole pointed nearly to the wall, as thus . At 7h. 0 it was the same. At 8h. 0′ it was the same. So concluded this was its natural position. At 9h. 30′ it was the same—so approached the bismuth and left it in the position marked all night .

9849. 10*th Octr.* At 7h. 45′ A.M. found the Magnet a little to the

right , as if tending to the equatorial part:—placed and left the bismuth . At 10h. 10′ A.M. the M. Pole had gone a little to the left and was very near to the magnecrystallic axis and to the chief angle of the piece of bismuth (   ):—left it until 10h. 30′ and was then still the same. Put the crystal round 45° to the right, and left it, but the magnet now rather far from the bismuth.

9850. At 1h. 40′ P.M. the Magnetic pole still where it was :—moved the bismuth on 90° and left it. At 3h. 50′ the magnetic pole was still in the same place —perhaps a very little to the left. Advanced the bismuth thus , so as to have the Magnecrystallic axis on the other side of the magnetic pole, and now if it has any power it ought to turn the pole to the right.

9851. 11 *Octr.* 1848. At 9h. 30′ P.M. the M. Pole was a little to the right , as if affected. Now by accident dismounted the bismuth and so left the Magnet to take its natural position.

9852. 12 *Octr.* 1848. At 7h. 30 A.M. The pole (alone) had taken up its position thus , the least possible to the left hand of the direct line from center of motion to the wall.

9853. I now mounted a *small* good crystal of bismuth on the stand and put on the jar with the crystal away from the Magnet, that the latter might take and shew its place of rest.

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9854. I brought the crystal of Sul. Iron and two of the crystals of Red ferro prussiate of potassa into my room and put them in the window so as to be subject to the action of light (9831). But I did not find that this caused any difference in the results except that more care was required to guard against currents. Hence I do not believe that[1] set either by the earth's action (in these latitudes and under my circumstances) or by the action of light.

9855. As to *repulsion of bismuth diamagnetically*, when its Magnecrystallic axis is either parallel to or perpendicular to the lines of magnetic force acting on it. I fastened a single thread of cocoon

[1] they?

silk at 2 points about 8 feet apart at the lanthorn of our lecture room, and let it descend to the lecture table, so as to make a looped, wedge shaped, bifilar suspension, about 28 feet in length, vibrating in one plane only. Then I suspended a crystal of bismuth from this suspension by a copper hook that should keep it in a given position and prevent rotation. A large cylinder Electro-magnet was then brought very near to the bismuth, so that in repelling it, it should send it off in the plane perpendicular to the plane of the bifilar suspension. Sometimes the bismuth was left in the air and sometimes sheltered by a thin glass tube closed below.

9856. Then observed the repulsion of the bismuth from the conical termination of the cylinder magnet when the Electric current was put on, and that when the Magnecrystallic axis was parallel to and also when perpendicular to the direction of the lines of magnetic force, and I could see *no difference either in its nature, direction or amount.*

9857. For Mutual action. Have arranged the large fixed and the small slung crystals of red ferro prussiate carefully in an inner vessel in a jar (9812, 9834) so as to have them perfectly free from currents of air, and now I can find no signs of mutual action, for in whatever position the upper one is, the least force of torsion put on is fully obeyed. There is no restraint on the one side or the other of the line parallel to the axis of the lower crystals, or in any other direction that I can see.

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9858. Have for many days past been engaged in examining the power of a crystal of stationary bismuth in affecting and moving a magnet. Used the arrangement before described (9432) and placed a small crystal of bismuth, about this size, on the central stand with its axis (magnecrystallic) horizontal—and on a level with the lower end of the suspended needle magnet. This bismuth could not only be revolved in the center of the circle described by the magnet (9433), but, by displacement of the lower end of the glass supporting rod, could be carried into any position near the needle and even round it if necessary, so that the bismuth and the magnet could in all the positions of the line joining their centers still be brought near together; for unless near, there was little hopes of any sensible action. Then to serve as an indication

of motion in the magnet, a mark was made on the glass jar containing the arrangement (a ball of cement was stuck on) in a line with the lever carrying the magnet and its counterpoise (9432), and on the side of the jar nearest the magnet. When the magnet was free from any other influence than that of the earth and the torsion of the threads, it, the counterpoise and this mark were in a straight line, so that any motion of the magnet to the right or left was shewn by an examination of these three points.

9859. Now the bismuth was brought near the magnet and successively placed in many positions in relation to it, so that the Magnecrystallic axis should either be parallel to the line joining the crystal and the Magnetic pole, or oblique to it more or less. It was always brought up to its position very slowly and carefully, and then the apparatus left for three or four hours that the magnet might come to its due state of rest. A second observation was taken in half an hour or an hour after to see whether it had moved or not, and not till this was satisfactorily done was a new position for a new observation given to the bismuth.

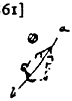
9860. All this was performed with the temperature very equal in the froggery, and with all care in respect of knives, spectacles or any thing else that could affect the magnet (9436). Wax candles on glass candlesticks were used and nothing that was iron was allowed to be moved about the place.

9861. The general result of experiments continued for five or six days is this—that the Magnetic pole tended to place itself end on to the Magnecrystallic axis of the bismuth. Thus, if it were naturally in the place represented ⊙ and the bismuth were thus situated, *a*, *b* being the Magnecrystallic axis, the pole moved to the left; or if it were thus, the pole also moved to the left. But if placed in the two following positions* the Magnetic pole moved to the right. This power was very small but sensible—it could carry the pole 60° or 70° or more aside from its natural position (9858).

9862. Still, the action was not quite regular; as if a little magnetic influence were exerted; and as the bismuth is an old crystal and been much used, it is very possible some magnetic dirt may adhere to it. Still, I am satisfied there is reaction of the bismuth on the magnet.

9863. Yesterday, tried many substances by the great Electro magnet, and amongst them most of the bodies from Mr Tennant.

* [9861]

 or 

9864. *Diamond* (9798) same as before. No pointing—or signs of Magnecrystallic or other action.

9865. *Sulphur*—a fine large crystal—an acute octoedron about 2 inches long by $1\frac{1}{4}$ and 1 inch broad—points equatorially by diamagnetic action. The result does not tell conclusively as to absence of Magnecrystallic action because crystal too large for magnetic field—but if it has M. C. power, it can only be in a small degree.

9866. *Phosphorous.* Portions of phosphorous were fuzed under water in glass tubes and left to cool slowly between the poles of the horse shoe magnet (9449). Being now tried at the great magnet, they gave no signs of any crystalline arrangement of particles, by the magnetic action or any M. C. power. See bismuth heated (9905).

9867. *Gold* (9799). Two fine crystals from De la Beche. Same as before, no M. C. action. The arrest action was good—the revulsive action none, for the form of the crystals approached too much to the orbicular shape and could not therefore give it.

9868. *Tin.* Dendritical crystals obtained by Electro chemical deposition (from Mr Phillips) were tried. There was no Magnecrystallic action—but the arresting and revulsive actions were there.

9869. *Oxide of tin.* The small long crystal (9803) examined—was a little magnetic. It pointed with its length axial, either between flat faced or pointed poles. No magnecrystallic action.

9870. *Lead.* Selected some good stout simple leaves from an old zinc or lead tree. They did well, i.e. they were pure and diamagnetic, also simple in their crystalline arrangement, but there was no signs of Magnecrystallic action. When the plate was vertical, there was plenty of arrest, revulsion, etc. etc.

9871. *Zinc.* Prepared two solutions, of sulphate and chloride of zinc. Put into each a plate of platina foil and a piece of pure zinc, and connected these with single pairs of Grove's plates for the purpose of transferring and depositing the zinc on the platina foils. After 24 hours, that effect had taken place.

9872. The zinc deposited in the sulphate adhered closely to the platina foil, making it bow or convex towards the opposite pole, and it had a mamillated or botryoidal appearance. A part of the compound plate of platina and zinc, of which above $\frac{3}{4}$ was zinc, was placed in the magnetic field; it was repelled as a diamagnetic,

but shewed no signs of Magnecrystallic force. Plenty of arrest and revulsion.
9873. The zinc deposited in the chloride solution was altogether different in appearance, consisting of beautiful dendritical forms, proceeding from the platina towards the opposite zinc, built up of innumerable small crystals, apparently solid but imperfect in form, the groups being an inch or even more in length and stiff enough to handle. This bundle I put into the magnetic field in three different perpendicular positions, but in none of them did it shew signs of Magnecrystallic force. It was diamagnetic.
9874. *Titanium*–from Sir H. T. De la Beche. These were crystals from the iron furnace, which I freed first by good digestion in dilute Sulc. acid–then fusion with mixed carbonates of potassa and soda, and then further digestion in mixed Sulphuric, Mur. and Nitric acids. They appeared to be quite pure and bright, but by boiling in N.M.Acid, gave up iron and titanium, both dissolving together, though only slowly. I believe iron is included in the whole mass of the crystals.
9875. They were *Magnetic*, being attracted, etc. They set powerfully and then acquired a magnetic condition which they kept, for when the electric current was off, they pointed by virtue of the residual force of the magnet. If held in any given position whilst all the magnetic force on, then the Electric current taken off and the crystal left free, it points as it was held, shewing that it had acquired a state (magnetic) which it can keep–this happens in whatever position the crystal is held: but if the crystal, magnetised in any of these ways, be left free when the Electric current is put on, it almost allways alters, and it always then takes on the magnetic state in one given constant direction, which is such that the greatest dimension is *not axial.* It is as if the crystals could all really become magnetised in any direction in which they are forcibly retained–but that one direction is more favourable for this disposition of force than another. This is probably connected with the crystalline character and the former results–but it may also be due to an irregular diffusion of iron in the mass (9935).
9876. *Oxide of titanium*, *Brookite* (9801), the same as before. It points but seems to be a little magnetic.
9877. *Tellurium.* Had two pieces, being fractured crystalline

pieces having well defined parallel planes of cleavage—the pieces were small—and pointed and were, I think, Magnecrystallic, for the greatest length was equatorial between flat poles; but I do not consider the evidence enough for a decision.

9878. *Iridium and Osmium.* A quantity of this substance was sheared and some of the large grains with plane faces selected—the chief planes are generally two, so that the pieces present the appearance of plates either thicker or thinner. Some are much Magnetic, others but little so. Found several good solid plates which, being very little magnetic, pointed with great force and always with their flat faces towards the poles. The grains were not above $\frac{1}{15}$ of an inch in the longest dimension, and yet they set vigourously when the poles were 3 or 4 inches apart; and whether so far apart or almost close together, always with the faces towards the poles—also whether conical or flat poles were use[d]. Believe they are Magnecrystallic. They correspond in some degree to the crystals of Sulphate of iron (9780).

9879. *Bismuth and Antimony* alloy in single proportion—breaks crystalline. Cast some small cylinders (9466) but on breaking them the cleavage was small and confuzed. The little cylinders were diamagnetic, but gave no signs of Magnecrystallic action—nor was it to be expected.

9880. *Fusible metal crystals.* Obtained a series by pouring off the metal in succession four times. The crystals were not so clean as those of antimony or bismuth and were apparently quadrangular prisms. They pointed, but with nothing like the character of antimony or bismuth.

9881. *Tin rod, amalgamated*—took a part of the four compartments of the rod and put it in the magnetic field. No M. C. action—arrest and revulsion—crystallization very confused however.

9882. *Wires.* Query whether in wires the crystalline particles are drawn into a parallel position, as the dissection of the surface of a platina, copper or iron wire might seem to indicate. A bundle of *platina wire* fine was tried, but it was magnetic and shewed signs of nothing else. A bundle of *fine copper wire* was tried; it also was magnetic and shewed no other signs, but in these case[s] the Mag. crystallic force might coincide with the line of draught, and thus would coincide with the magnetism.

9883. A bundle of eight strands or pieces or lengths of thickest tin wire was made up. It also was magnetic and gave no other distinct results than those of magnetism.

9884. *Plates of Tin foil.* A bundle of 64 plates of tin foil put into the magnetic field. It shewed nothing resembling Magnecrystallic action. But the phenomena of *advance*, *arrest*, *revulsion* were shewn in perfection. A bundle of copper plates would do this admirably.

9885. *Boracites* (9800). The two pointed feebly between flat poles—think it was magnetic, but had not time to examine further.

9886. *Ruby silver ore* (9805). If the axis of the prism vertical, there was hardly a trace of pointing, yet one horizontal direction larger than the other. If the axis were horizontal, the piece pointed well and with the axis of the prism axial in the Magnetic field. Yet this was not the greatest length of the mass. I incline to think the phenomenon was Magnecrystallic in character.

9887. *Iron pyrites* (9808) as before—i.e. a trace magnetic but nothing Magnecrystallic.

9888. *Arsenical Iron* (9809)—as before—points, but irregularly.

9889. *White Cobalt* (9811)—as before—points uncertainly.

9890. *Iceland Spar.* My small cube (   ) points well between the flat faced poles with the optic axis equatorial. A small rhomboid also points equally well and clearly.

9891. *Sulphate of Manganese.* Crystals small and bad—flat rhomboidal prism apparently—very magnetic. No signs here of Magnecrystallic action.

9892. *Sulphate of Manganese and Ammonia.* It is Magnetic, but I believe is also Magnecrystallic, but not having good crystals, only record this as a preliminary result to further experiments.

9893. *Sulphate of Iron* (9780). Beautiful—points at any distance less than 8 inches or perhaps much more—is attracted to either pole—but length is always equatorial with ordinary crystals. As I have bundles of crystals 4 or 5 inches long, can make some good experiments with this body—as to mutual action—evolution of currents, etc. etc. (   ).

9894. *Sulphate of Nickel* (9783). Magnecrystallic, just as before—but well and strong.

9895. *Pure sulphate of Iron and Ammonia.* Magnetic. No clear Magnecrystallic indications.

9896. *Yellow ferro prussiate of Potassa.* Indifferent, Diamagnetic.

9897. *Selenite* Sul. lime. Not Magnecrystallic.

9898. In some of these experiments found the suspending loop a little magnetic—that should always be watched when there is reason to suspect that it causes the effect attributed to the substance; it is easy, by setting the suspension on a quarter round so as to bring the loop into another place, to check that notion.

9899. *Heat on bismuth crystal.* A crystal of bismuth was suspended by a long copper loop and stem, so that the heat of a small spirit lamp could be applied to it without affecting the silk suspension above. In the magnetic field it was well affected and set so vigorously that the lamp flame when applied beneath did not disturb it. The temperature was carefully raised and slowly and the Magnecrystallic power of the bismuth continually observed. At last this ceased altogether, or so much that the crystal revolved by the force of the ascending current as if not magnecrystallic. The heat or lamp was instantly removed, but it was seen by a minute globule of exuding melted bismuth that the piece was just at the fusing temperature.

9900. As the temperature fell, the bismuth regained its Magnecrystallic force and I think in a full degree.

9901. Hence the bismuth looses its power at a temperature just short of the fusing point—but before it has fuzed; and regains it again as the temperature falls.

9902. On breaking open the crystal when cold, it was found that the interior was all regularly crystallized, for the cleavage planes ran across as if the crystal had not in that respect been touched.

9903. Suspended another crystal of bismuth in oil and applied heat to the oil whilst all was in the magnetic field. Here again the bismuth pointed well Magnecrystallically as the heat rose—but at last it lost the power just as a drop of bismuth oozed off at the bottom of the crystal and sank thrgh. the oil. Removing the lamp, the bismuth remained indifferent longer than before, for the heat of the oil continued and the bottom of the bismuth became rounded from a gathering drop—but as heat did not continue, the temperature of the whole gradually fell and then the

piece resumed its Magnecrystallic power as before, but not so strongly.

9904. When cold, the piece was broken and it was found that the lower part, where the drop was, was confuzedly crystallized, but that part of the crystal remained all the time unmelted. This accounted for the diminution of power in the end, and still shewed that the crystalline bismuth lost its Magnecrystallic relation *before* it lost its solidity.

9905. As bismuth loses its power before it fuzes, so cooling bismuth does not receive its power until after it is solid; hence there is no reason to expect that bismuth, by congealing in the magnetic field, would crystallize regularly, and be as a mass Magnecrystallic.

9906. In reference to mutual action of crystals, I have put the small crystal of red ferro prussiate (9812) by the side of the larger one in various positions, but it did not seem affected. The crystal always agreed in position with the torsion index.

9907. Two short thick tourmalines have been suspended or arranged in a jar, one over the other as before (9834), but there seems to be no clear evidence that the one can influence the other.

9908. Two longer tourmalines (9834) were similarly arranged, with the same negative result.

9909. Try mutual action by sulphate of iron crystals.

9910. As in Magnecrystallic phenomena the Magnet acts on the crystal, so the crystal also acts on the magnet, and moves it as in the suspended magnet.

9911. Now how does the crystal react? It is not magnetic as iron is, and if in an induced state, what is the kind of action which makes the pole go round it so far?

9912. How are the lines of magnetic force affected backwds. in this action and what are these lines? Are they lines of contiguous acting particles?—or are they like lines of gravitating force or rays of light?

9913. The original state of the crystal is *Magnecrystallic*—but the induced state ought to be distinguished as the *Magneto crystallic* condition.

9914. I do not remember heretofore such a case of force as the present one, i.e. a bringing into position only, without attraction or repulsion.

9915. Extraordinary character of the force—not polar for no attraction or repulsion.

9916. Then what is the mechanical force which turns the crystal or makes it move a magnet?

9917. Is it like the case of the tangential motion in the rotation of the Magnetic pole and wire, in any respect—that, according to Ampère, consists of the two forces of attraction and repulsion?

9918. It is not like a turning helix acted on by the lines of magnetic power, for there there is a current of Electricity required, and the arrangement has poles all the time, and is attracted or repelled.

9919. If we suppose that the axial is the unaffected condition and that it is in the oblique position that the ends of the axial line (M. C.) are polar, giving two attractions pulling the crystal round—then there ought to be attractions at those times, and an obliquely presented crystal ought to attract if nearer one pole than the other—but there appears to be none.

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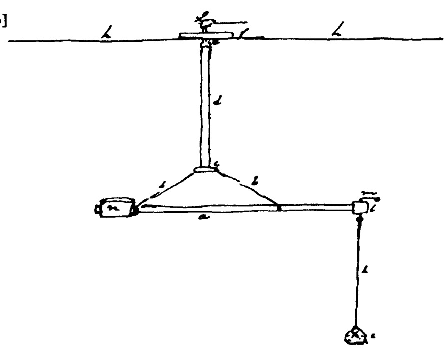
9920*. Arranged to try the repulsion of a bismuth crystal in two directions, using a bifilar torsion balance of the following arrangement. *a* is thin small rod of wood 5 inches long, sustained by two copper wires *b*, *b* made fast to a card *c*; to this is attached two bundles of silk cocoon fibres of eight each, *d*, so as to produce a bifilar suspension, the bundles being about $\frac{1}{12}$ of an inch apart and about 4 inches long. These were made fast to a nut and pin *e*, working in the center of the divided circle *f*, and to the pin at the head *g* was attached a torsion index. *h*, *h* is the top of the glass box which sustained the apparatus, the sides of which also were of glass and perpendicular. *i* is the crystal of bismuth held fast

* [9920]



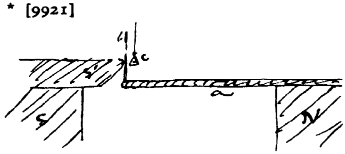
in the loop of the copper wire *k*, which is also sharply looped above. *l* is a piece of cork fixed on to the end of the long arm of the lever *a*, and *m* a pin, which passing vertically thrgh. the cork, is sharply looped below to receive the loop of *k* and bent to an arm above in a horizontal position by which it can be turned 90° or any other quantity. The loops of *k* and *l* are so sharp that though they allow *i* to hang perpendicular, still every motion given to the hand of *m* is followed instantly by the crystal *i*, so that it can be turned into any position without loosing its perpendicular place.

9921*. The bismuth was by the Index above brought close up to but not touching the side glass of the case, and a scale of marks was put under it to shew where it stood at any time when repelled. The apparatus was arranged over one of the ends of the great Electromagnet from which the moveable pole was removed, and the other pole had its pointed moveable piece placed with its end close against the glass opposite to the bismuth. S and N are the great poles, S′ the smaller pointed pole—*a* the wooden bottom of the glass box—*b* the side glass—and *c* the bismuth. So that the lines of force proceeding from the end of S′ diverged, as it were, and weakened rapidly towards N, and in this diverging Magnetic field the bismuth *c* was placed.

9922†. In order to observe the place of the bismuth before and after its repulsion, the place of the suspending line prolonged to the scale was observed, for as the bismuth was irregular in shape and had to be revolved, its parts would not serve. So in plan, S′ represents the pointed pole, *g* the glass side of the box, 1, 2, 3, 4 the Scale, *b* the place of the bismuth, and the dots the place of the projection of the suspending line, without and with the magnetic action, the difference shewing the repulsion.

9923. At first the bismuth was with the pointed end towards the Magnetic pole and the Magnecrystallic axis coincident with the axial line of the magnetic field that was to be, and its place was marked in relation to the line 1 of the scale by the left hand dot i. Then the magnet was thrown into action by ten pair of Grove's plates, and by proper touches the bismuth repelled and brought to rest; it stood at the right hand dot i—the difference measured the repulsion in the force of twist in the bifilar suspension.

* [9921]



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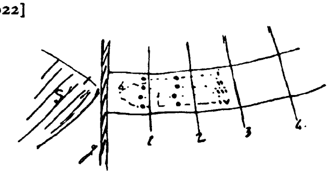
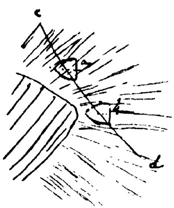


9924. Then the pin *m* (9920) was turned 90°, so that now the bismuth was thus, with its Magnecrystallic axis perpendicular to the lines of magnetic force: the positions ii shew the repulsion in this case.

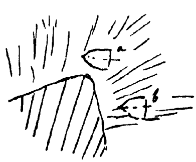
9925. Then the bismuth was turned successively into positions 90° further advance, as*, and the degree of repulsion shewn by iii and iv. In iii the bismuth was the least degree nearer to the magnet, but the proportionate repulsion is the same—and the general result is to me that the repulsion of the bismuth as a diamagnetic body is the same, whatever the position of the Magnecrystallic axis to the lines of magnetic force may be..

9926. As an indication of the force counteracting the repulsion, I may record that a vibration in one direction occupied 9″, or a vibration to and fro 18″.



9927. Desired to see by experiment if a crystal of bismuth were suspended so that it could move in the line *c*, *d*, and were presented to the magnetic pole so that at *a* the Magnecrystallic axis would be across the lines of magnetic force, but at *b* be parallel to them, whether it would move from *a* to *b* or not. According to the effect with a suspended magnet (9858–61), it should—as a diamagnetic body it should not, for *b* is a place of stronger action than *a*. So altered the position of the bifilar arrangement in such a manner that the bismuth could move along the line *c d*, there being only room for the glass of the case between it and the magnetic pole, and the bismuth was turned in the socket *l* (9920) so that its magnecrystallic axis should be as represented. The position *a* was the place of rest.

9928. Now on making the magnet active, the bismuth went slowly from position *a* to that of *b*, bringing the Magnecrystallic axis into parallelism with the direction of the Magnetic lines.

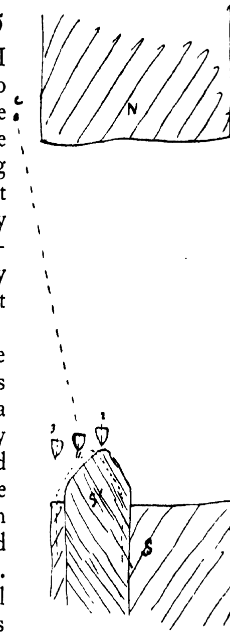


9929. In order to compensate for the opposing effect of diamagnetism, I now moved not the suspension or the bismuth but the magnetic pole, placing it thus, and now on making the magnet active the bismuth went from *a* to *b* and with considerably more force than before, for now diamagnetic and magnecrystallic action were conjoined.

* [9925]

 and 

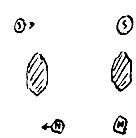
9930. In another experiment to separate diamagnetic action I arranged matters thus: S and N are the large poles of the electro magnet (in plan), S′ is the pointed pole used to influence the bismuth, *c* is the center of suspension of the bifilar balance, 1 is the place of rest of the bismuth crystal, its magnecrystallic axis being indicated by a short dotted line. Now by diamagnetic action, it should go from 1 towards 3 as a weaker place of action–but by Magnecrystallic action expected it would go towards position 2–and so it was, for when the Magnet was active, the bismuth slowly went up to 2 and continued there as long as the electric current was continued.



9931. When the pole S′ was moved to the left, so as to be opposite position 1, then on making the magnet the bismuth went towards 2 and reached it and stopped there. When the pole was moved a little to the right, so as to point between 1 and 2, as shewn by the dotted form, then on making the magnet active, not only did the bismuth go from 1 towards 2 but, when it had come opposite the pole, it[s] motion was accellerated and it went beyond 2. In the first part of its course, diamagnetic action was against, and in the second part of its course with the Magnecrystallic action.

9932. These motions must of course have relation to the conical form of the lines of magnetic force in the Magnetic field. It is not possible, I think, that a bismuth crystal could move parallel to itself across lines of equal magnetic force, as between two flat faced poles near to each other. There is no reason why it should move one way more than the other–its tendency there is to turn in position.

9933. But as a crystal turns between two mag. poles, so, if it were fixed and the poles mobile, they would tend to turn about it, and it is this tendency which makes the suspended magnet move about the bismuth (9858–61). As a consequence of that motion (or its cause), the motions I have just described were expected and looked for, and ought I think to follow.



Heat on Antimony.

9934. The group of crystals No. 12 (9631) were hung by copper wire and heated by a small spirit lamp flame in the open air between the conical poles of the great electromagnet, as the crystals of bismuth had been (9899). Whilst cold, they pointed

well, taking their position at once by reason of arrest action (9632), and with the length of the mass across the axial line.

9935. On heating the piece, this power disappeared on the temperature acquiring a dull red heat—and then whilst hot the piece was magnetic, pointing with its length axial, both whilst the electric current was on and off, and being strongly attracted when the current was on. The magnetism always was in the direction of the length, unless the piece were held in some other position (as with titanium (9875)), and then it took for the time the new direction, but when the Magnet was restored to full power, the magnetism returned to the former or long direction. Hence the piece is quite altered in its nature.

9936. During the process, the heat had been raised to such a degree as to produce abundant fumes of oxide and to colour the flame of the lamp, and the piece had been so softened, that though the upper part kept the crystalline form of the cubes (assisted perhaps by the rigid coat of oxide on them), the lower part had sunk down, so that the copper wire going round it had formed a deep line in it.

9937. When cold it broke crystalline throughout, and the upper crystals were crystalline; removed, by grinding, the black, under, and most heated and altered parts, but the upper part, which was apparently unaltered in shape and appearance, still behaved when cold as the titanium (9875) and as the whole piece.

9938. I ground off at both ends of the length so as to make that given direction a shorter dimension than the others. Still the new longest dimension was not that which pointed axially but a position between.

9939. The piece is now too small. The magnetic effect is no doubt due to iron, which had before been over-ruled by Magnecrystallic action. I think the temperature must have been nearly up at the softening or melting point before the Magnecrystallic action was lost.

9940. *Iceland Spar heated.* A small rhomboid of Iceland spar, clear and transparent, was similarly suspended (9934) and subjected to the heat of the spirit lamp until the copper wire was full red hot and even the edges of the rhomboid, though the solid transparent part was not visibly ignited. Still, it must have been

as hot as red hot copper, and also as hot as the flame of a spirit lamp could make it. But all this time, it pointed with its optic axis equatorially and vibrated about that position, just as well as if it had been cold.

9941. *Tourmaline* heated. A short thick tourmaline was similarly suspended (9934) and heated between flat faced poles. It was raised to a fair red heat and as high as the spirit lamp could raise it, but always pointed with the optic axis equatorially and equally well. Hence its Magnecrystalline power is unaffected by such temperature.

9942. On cooling however, it began to point axially and soon did so very strongly, and was very magnetic, being attracted to the magnetic poles. It did not seem to be electric and did not attract a suspended gold leaf. When hung up endways, it was also strongly attracted and appeared to be of no use in Magneoptical experiments. But observing that it was the part which had been underneath and was therefore exposed to the heat and deoxidizing vapour of the spirit lamp that was most strongly attracted, I suspected that some per oxide of Iron had perhaps been partly reduced. I therefore digested the tourmaline for a few minutes in N.M. acid, which instantly shewed by its colour the removal of iron; and now when washed, it had lost all the extra magnetic force, and was in its first condition, pointing optically as before.

9943. *Large crystal of Sulphate of Iron*—a group 5 inches long hung up by bobbin. It did not point Magnecrystallically, but Magnetically. Cut off the base where the parts seemed confuzed, and found that the inside was full of confuzed loose crystals—all being covered over with a well crystallized jacket that had good form externally. Cleared out the inside and then the outside jacket pointed Magnecrystallically. These will do for expts. to a large audience.

9944. The *magnecrystallic axis* in Sulphate of iron crystals appears to be in a line perpendicular to two of the side planes of the rhombic prism.

9945. I had a small rhombic prism crystal whose length was $2\frac{1}{2}$ times its breadth, but whatever formed poles I took or however near—or whether the crystal was between or above the two poles, I could in no way make the length point axially if the Magne-

crystallic axis were in the horizontal direction, so great is the Magnecrystallic power. I took other clear good crystals, but in no case could I by mere magnetic force overcome the Magnecrystallic force.

9946. When the Sulphate of Iron crystal was in the magnetic field, I could not perceive any sensible influence of another crystal of the same salt; brought it near it in different positions.

9947. The *double sulphate of potash and nickel*—points Magnecrystallically.

9948. *Sulphur*—a piece of a large clear crystal of native sulphur pointed Magnecrystallically, having an equatorial resultant of force; i.e. when most affected, that line was in the equatorial direction which, when turned upwards, left the piece nearly indifferent. Probably the optic axis is there. Make a cube and look for it. The substance points nearly as well as Calcareous spar. By further working and experiments, obtained uncertain results with same pieces, and rubbing them down to different dimensions, believe effects to be diamagnetic.

9949. *Amalgam of Tin.* When an amalgam of tin is melted by heat in about its bulk of mercury, and left to cool and crystallize, mercury can be poured off, and the mass which floats is found crystallized. The crystals at the lower part are plates which, when drained, may be separated, and though wet with and soaking in Mercury, can be examined at the magnet. They were diamagnetic and I think Magnecrystallic, but require more careful examination and especially attention to the suspender, for their power if they have any is weak.

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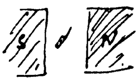
9950. *Set of Iron Crystals.* A strand of 15 cocoon filaments about 14 inches long was made fast above, and below had a piece of lead hung by a wire to it to keep it taught. This piece of lead rested against the side of a wooden block, low down beneath the cheeks of the Electromagnet, so as not to spin or swing but give a steady axis of silk thread, the middle of which passed through the magnetic field. A small piece of card was fixed across this axis by sealing wax, and then a prismatic crystal of sulphate of iron was fixed on one side of the cardboard, so that the length

and also the Magnecrystallic axis should be horizontal (   ), and the crystal when rotated in a horizontal plane move entirely round the silk axis, which in fact is at one end of it. It tended of course to occupy a given position, due to the torsion of the bundle of filaments confined both at the top and bottom, and this torsion force was such as to make the crystal in its swing complete a double vibration in $\frac{1}{40}$ of a minute.



9951. When the crystal was between the two flat faced Mag. poles, and arranged so as to stand obliquely to them when at rest: on making the magnet active, the crystal moved, tending to place itself with its faces parallel to the pole faces and the Magnecrystallic axis perpendicular to them or parallel to the lines of Magnetic force. When N was taken away and the Magnet again made active, the same direction of motion was given to the crystal, but not to the same extent as before. On bringing the crystal as near as could be without touching to S, then the motion was the same as before but greater.

9952. Now in this experiment the crystal, though a magnetic body, actually receeds from the pole of the magnet by the influence of the magnecrystallic force, and therefore I conceive it is impossible to suppose set due to any extra induction of Magnetism in the direction of the Magnecrystallic axis (10006).

9953. If the pole N be retained for action on the crystal, then the latter is sent towards the pole with increased force, both by reason of the magnecrystallic force and the magnetic force. The experiment requires care but was perfectly successful. Conical poles did not answer, being apparently too powerful in inducing ordinary magnetism, and the diminishing power of the curves of Mag. force seems to aid this effect.

9954. I replaced the crystal of Iron by a plate of bismuth, which under the same circumstances manifested a tendency to approach the pole when related to it as N is at (   ), and where therefore the tendency of the Magnecrystallic force to counteract the diamagnetic force was evident. But the effect was not so good as with the Sulphate of Iron.

9955. I ground down a crystal of red ferro prussiate of potassa into a plate and applied it in the same manner, the whole length being on one side of the axis and the plane of the optic axis vertical.

This, when standing obliquely between the two flatfaced poles, moved and placed itself equatorially. When pole N was removed and S brought up ( ), the same motion occurred: the crystal then as a body receeding from the pole. But when pole S was removed and N brought up, it moved as before, the whole body approaching the pole. Hence the appearance of attraction and repulsion of the same body by the two poles, and on turning the crystal so as to incline it on the other side of the equatorial line, the phenomena were repeated in the contrary repulsion.

9956. Hence the appearance of attraction and repulsion by the same pole and by both poles—and hence the proof that attraction and repulsion is not the cause of the set and final position of the crystal and the other bodies.

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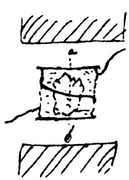
9957. The Bismuth crystal was placed in the concentrated Magnetic field of the large ordinary horseshoe magnet ( ), sometimes with its Magnecrystallic axis equatorial, and at other times axial or oblique; and then it was touched in different directions, as at the ends of the Magnecrystallic axis, or the sides, or end and side, and in all directions, by the wires of my delicate galvanometer, but no signs of any current were visible, the crystal being at rest. The crystal was No. 6 (9548). Great care is required to avoid thermoelectric effects, and the ends of the Galvr. wires must not be held in the hands, touching the skin. I used cork handles. So there is no current produced in the oblique position of the crystal.



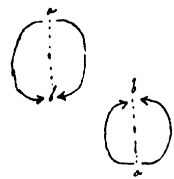
9958. Bismuth crystal No. 6 (9548) was surrounded by a small helix of covered fine copper wire containing about 10 feet in length, with the Magnecrystallic axis in the axis of the helix. The direction of the M. C. axis is shewn in the figure by the dotted red line, and the direction of the helix wire by the red line[1].

9959. When this helix and crystal were connected with my galvanometer wires and then put into the Magnetic field of the horseshoe magnet, a current was produced at the Galvanometer; when it was taken out of the field, a contrary current was produced.

[1] In the original sketch the vertical dotted line and the waved line encircling the crystal are in red.

But when a corresponding helix without the bismuth crystal was connected in the same way, and introduced in the same way to the magnetic field and also removed in the same way, precisely similar currents in direction were produced. So that Bismuth added nothing apparently and certain[ly] cause[d] no variation in the direction of the induced currents.

9960. When the Bismuth crystal and helix were placed in the magnetic field in the position figured above and the Galvanometer at rest, then motion to the right *or* left of the line *a b* in the direction figured and through the whole or any part of 180°, gave a certain current at the galvanometer, always in the same direction. But when *a* and *b* had changed places, then either the motion onwards or backwards gave a *contrary* current at the galvanometer.



9961. When the helix only (9959) in the correspondant position (9958) was moved in the same directions and to the same extent, exactly the same currents were produced. So here the bismuth crystal seems to change nothing. If it adds to or subtracts from the power of the current induced, the change is very small.

9962. The currents are exactly such as ought to be produced by a moving wire according to the law laid down in my first series of Exp. Researches.

9963. My galvanometer stood so that about 1 o'clk. the sun's rays fell on it, and nearly parallel to the Mag. needle. In the course of a short time, the needle was deflected from 0°, 35° or more. On sheltering the whole instrument, this went down; on sheltering the coil below and not the graduated plate over it–it did not go down. On sheltering the plate and not the coil, it did go down or diminish. So the effect seems to be due to the sun's rays and the plate. Now this plate is, in the middle, copper to damp the vibrations of the needle, and at the edge where it is graduated, brass silvered, and I think the deflection is probably due to a thermo current produced by the sun's rays heating the one or near part of this system more than the further part–from the accidents of construction.

9964. Still, there were some appearances which seemed to shew that the plate, or plate and coil, had become as a whole magnetic, for the needle would stand at moments either on the one or other side of zero.

9965. Joining or separating the ends of the Galvanometer wires did nothing–so that it does not seem as if the effects depended on any current in the coil.

9966. Large group of Bismuth crystals which pointed fairly had the ends of the Galvanometer wires tied against extremities of the Magnecrystallic axis. It was then put in the magnetic field in various positions, and also moved in different directions, but no signs of any electric current generated in the moving Crystal, whether moving with or against its natural tendency, appeared.

9967. The conducters were then made fast against the ends of the line transverse to the Magnecrystallic axis in two directions–also to the equatorial and polar extremities at once, and in various other positions, but no current was generated in the magnetic field whether the bismuth crystal were still or moving.

9968. Two bismuth crystals were arranged in the Magnetic field, an apex of one, being the end of the Magnecrystallic axis, touching the equatorial parts of the other, and then the Galvanometer wires were made to touch them in different places and in different positions. But no result of wire contact under these circumstances appeared in the form of a current.

9969. Great care required as to the warmth communicated to the copper wires of Galvanometer; for one, being thicker than the other, conducted more warmth to the bismuth and that made a thermocurrent. Also touching the copper with the hands almost always generated an electric Voltaic current–though both wires were covered.

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9970. 2 oz. measure of Strong Oil of Vitriol and 1 oz. measure of Absolute Alcohol were mixed–and then a portion put into a small bottle, No. 1, and the rest into a larger bottle, also Number 1. Then Nine fragments of a broken discoloured diamond weighing altogether 0·7 of a grain were put into the larger bottle and the stopper being then tied over. All was left for the action of time.

9971. 2 Vols. of Strong Oil of Vitriol and 1 vol. of ordinary Alcohol were also put into two similar bottles marked No. 2 each and 9 fragments (also [illegible]) and of the same diamond were put into the larger of these two–they weighed altogether 0·7

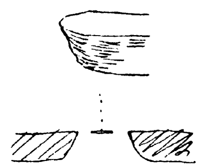
grains. These bottles were tied over and put away (15776), Aug. 1858.

28TH NOVR. 1848.

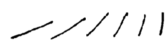
9972. A prismatic crystal of Sul. Iron suspended with length and Magnecrystallic axis both horizontal, when placed between the poles of the horseshoe magnet, pointed with the length axial and the Magnecrystallic axis equatorial, which to me was unexpected, as I think at the Electromagnet it would have pointed with the length equatorial. To ascertain whether this depended on the strength of the magnet–other experiments were made at the Electro magnet, of which the following are results.

9973. The crystal which acted thus at the horseshoe magnet was a prism $\frac{7}{8}$ of an inch in length, about $\frac{1}{8}$ broad and $\frac{1}{16}$ in thickness, and the Magnecrystallic axis coincided with the thickness.

9974. The magnetic poles used were the pointed ones (   ) and placed horizontal thus*, as seen in plan.



9975. Now it was found that when the crystal was suspended between them and level with the upper surface–therefore in the line joining the apices or points of the pole[s]–if the poles were less than a certain distance apart, the crystal pointed with length axial; if the distance apart were more than a certain quantity, it pointed length equatorial, and with a *certain small range of distance* it had an unsteady position, standing oblique at intermediate distances and so passing from this —— by steps to the equatorial position.



9976. Whether 1 pr. or 10 pr. of Grove's plates were used to excite the magnet, these distances and the corresponding effects of pointing remained apparently the same–so that variation in the strength of the magnet does not seem to affect it; but with the stronger magnet, it is strongly attracted to the one or other pole, unless retained on a vertical axis held both above or below.

9977. The intermediate distance or neutral distance may be called *n*; in the present case it was about $2\frac{1}{4}$ inches–but doubtless varies with different crystals.

9978. When at the *n* distance, if the crystal be lowered a little, it sets more obliquely, and lowered still more it sets length transverse. If it be raised also above the horizontal line (9975), it

* [9974]

quickly sets round and becomes quickly transverse. It, as was to be expected, attains this transverse position quicker in rising above the poles, than in sinking below them between the chops of the magnet. I doubt whether using 1 or 10 pr. of plates in the battery causes any change in the effect of elevation or depression. The result does not seem to have much, if any, dependance on the strength of the Magnet.

9979. When the distance of the Mag. poles is greater than *n* (9977) and the crystal sets length equatorially between them—then whether raised or lowered, or with 1 or 10 pr. of plates, the position retained is still and always the equatorial.

9980. When the distance of the poles is less than *n*, as for instance about $1\frac{1}{2}$ inches, and the crystal therefore pointing length *axially*, if lowered a little it still did the same but with less force. As it was thus gradually lowered, it lost more and more of its axial pointing power, and when about $1\frac{1}{2}$ inches below the upper level of the poles, it was in the uncertain state; and below that again, it pointed with the length equatorial or by Magnecrystallic force. This was the case for the different distances, whether 1 or 10 pr. of plates were used to excite the magnet.

9981. When it was raised above the level of the poles, the same series of effects occurred, but far more rapidly; and when the elevation was only half an inch, the crystal was nearly length equatorial. Whether 1 or 10 pr. of plates was used the effect was the same.

9982. At greater heights the set was length equatorial, and this took place when the crystal was $4\frac{1}{2}$ inches above the level of the poles and they only $1\frac{1}{2}$ or $1\frac{3}{8}$ apart.

9983. Another crystal of the Sulphate of Iron was [? whose] length was $\frac{11}{16}$ of an inch had $1\frac{5}{8}$ inches for the *n* distance or interval of the poles. At $1\frac{6}{8}$ interval, it set well equatorially. All the effects of raising or lowering were in the same order as before.

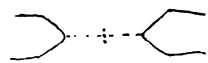
9984. A still shorter crystal had the *n* distance only $1\frac{1}{8}$ inches.

9985. With the same crystal, I have no doubt that the *n distance* will be shorter for flat-faced poles and longer for conical poles.

9986. The following circumstances probably affect the results:

The *n distance*—*increased* by pointed and diminished by flat faced pole.
„ „ *increased* by increase of length in the crystal.
„ „ *diminished* by increase of length of Magnecrystallic axis.
„ „ *variable* with different substances.
„ „ *not affected* by the force of the magnet.
„ „ *not affected* by the vertical depth of the crystal.

9987. When with the long crystal (9973) the distance was made *n* or about $2\frac{1}{4}$ inches, the crystal set obliquely across the line of the magnetic axis, and it would also take the diametral position, but not the other . Being then turned 180° so that its lower edge became the upper, it as expected set thus  and could also take the diametral position. This effect was constant whether the point of suspension was in the exact axial line or to the one side or the other, so as not to depend on the irregularity of the position but on something in the crystal itself. A little irregularity of form in the crystal might be the cause of it, as thus ; but I think there was hardly enough difference of that kind to produce this effect. A little obliquity of the M.C. axis might be the cause, but it is worth making out. Perhaps the horse shoe magnet may do for it. The use of 1 or 10 pair of plates made no difference in the result.



9988. Sometimes I thought I saw a tendency in the crystal to keep a state with a certain degree of force. Still, it does not bring any state away from the Magnet when removed. So this can hardly be.

9989. When a copper plate is moved saw fashion between the poles and then the Electric current put on—the manner in which the magnet grows up in force is easily felt and proved. Here it could not be any state of the copper, because the copper is continually changing its place all the time.

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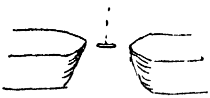
9990. Worked with Electromagnet and Sulphate of Iron crystals.

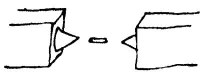
9991. Examined various crystals of *different* crops—often mistaken—for though those of one crop are similar, yet those of different crops vary. Thus sometimes the Magnecrystallic axis is in the breadth and not the thickness of the crystal, and once or

twice I found it in the length, nearly. It was always consistent and the reason of these apparent variations is only that, in different cases, one of the crystalline dimensions of a crystal grew faster than another.

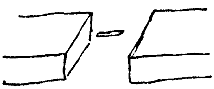
9992. Took the crystal (9973), being a prism $\frac{7}{8}$ of an inch long, and suspended it in one uniform direction between differently formed poles. Thus, between these poles and level with the top surface, the *n* distance was from 2 to $2\frac{1}{4}$ inches. At 2 inches it was inclined about thus in reference to the magnetic axis. Used 10 pr. of Grove's plates.



9993. Poles being cones in square flat faces and the crystal level with the axis of the cones. The *n* distance was then only from $1\frac{6}{8}$ to about 2 inches. For though the cones sharper than the former poles, yet the crystal is now more down, as to the whole magnetic field (9978). The oblique position occurred as before, and was also diametral.



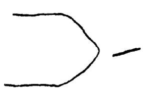
9994. Poles being large thick plates with flat faces, and the crystal level with the top surface. The *n* distance now smaller still, being from $1\frac{1}{8}$ to $1\frac{4}{8}$ or $1\frac{5}{8}$ inches. The inclination still as before .



9995. As the form of the pole passes from the more acute to the more flat, i.e. from the form more favorable to that less favourable to ordinary magnetic development, so the *n* distance diminishes, as it ought to do.

9996. With the smaller distances, it was necessary to use only 2 or 3 pair of plates, as the vertical axis of suspension was not confined below. Else the ordinary magnetic attraction took the crystal to one or other pole. But the distance *n* did not seem to be altered by that (9976, 9986), and I think that the oblique set of the crystal was not altered in its amount.

9997. When only one of the pointed poles (9987) was used, the other pole being the great end of the magnet, the same effects were produced by different distances from that pole, and also the same oblique set within the *n* distance. When the distance of the pole was about $\frac{5}{8}$ from the end of the crystal, the latter was nearly axial; now $5 \times 2 = 10 + 7 = \frac{17}{8}$, which is within the *n* distance, when 2 poles were used.



9998. I now put another crystal by the side of the first, so as virtually to increase the Magnecrystallic axis dimension, that being

now in the thickness of the two. The *n* distance was less than before, being from $1\frac{6}{8}$ to 2 inches. As it ought to be (9986).

9999. Proceeded to examine the cause of the oblique set of the crystal within the *n* distance. The crystal therefore was suspended between the poles, at such *n* distance as gave about this position  for the set—and this was diametral and equally steady and determinate in the two cases.

10000. Then turned the crystal 180° on its length as axis, and now the crystal pointed thus;  i.e. oblique as much as before, but on the opposite side of the axial line.

10001. Then turned the crystal 180° round its magnecrystallic axis, and this also caused a change in the *direction* of the oblique set, but the amount was as much as before.

10002. If I turn it 180° round a *vertical* axis, then it ought not to alter the obliquity, for that is only the diametral position—*and it does not.*

10003. A second, and a third prismatic crystal, of different lengths and thickness, when within their *n* distance, shewed all the same phenomena. All the positions were diametral.

10004. All these phenomena indicate an oblique resultant of action, and would be explained fully either by assuming that the Magnecrystallic axis is not perpendicular to the faces of the prism but oblique in the direction of its length, or that the maximum line of ordinary magnetic force is not in the axis of the length but oblique to it. For it is manifest that the resultant of the two forces is, within the *n* distance, oblique to the length of the prism.

10005. This effect might easily be measured in cubes cut from the prism or in other ways, but would take time. As it vanishes nearly in the distances greater or less than *n*, it is probably small—being here evident by the power we possess of making one or the other predominant at pleasure, and so having a very nice adjustment. Hence the *n* distance may turn out a very useful means of investigation.

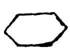
10006. Repeated the expt. with the vertical axis and a crystal to be removed (9950, etc.). It is not difficult so to adjust matters as to obtain the set and the recession, but not to any great degree, for the ordinary magnetic action is strong. Still, by using a blunt or round faced pole, and that not too close, the forces are easily adjusted and the effect produced.

10007. Took a crystalline fragment of bismuth with a good bright cleavage plane perpendicular to the Magnecrystallic axis. Placed this in its *natural position* in the magnetic field, and then sent a polarized ray from different angle on to the cleavage plane and observed it by a Nicol Eye piece as analyzer. No difference in the results occurred, whether the strong magnetic force of the battery was on or off. So no result of action on the reflected ray.
10008. Besides polarization—a film of Sul. lime was introduced but with no good effect.

18 DECR. 1848 at Brighton—fine sunny day—sun low.

10009. The small cube of Iceland spar (Exp. Res., 1695) of about 0·4 inches in the side was suspended, in a Jar and sheltered by an inner flask, by a single cocoon fibre; and so that the optic axis was horizontal and two planes parallel to it also horizontal. It was then brought to rest with a diagonal parallel to the sun's rays as they entered from the south thrgh. a window. So two planes received the rays at angles of 45°, and one of these was perpendicular to the optic axis and the other parallel to it. Now whether the sun's rays were allowed to fall on to the cube, or they were stopped off by a screen, still the Cube preserved the same position. So the light caused no tendency to set under these circumstances.

10010. A Nicol's eye piece was then fixed in an opaque screen and placed before the cube, so that the polarized sun's ray only should pass and fall on to it. The field of sight in the Nichol had this shape and position, so that the ray was polarized like that which would be *transmitted* by a plate of glass reflecting in a vertical plane, or unlike the reflected ray. No tendency to motion or set was evident when this ray fell on to the faces of the cube.



21 DECR. 1848. Brighton.

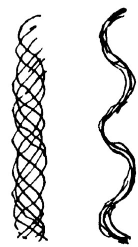
10011. Fine day. Sunshine, and Expts. of same kind continued.
10012. Whether the sun's ray was polarized as before (10010) or in the plane at right angles to that direction, still there was no effect.

10013. If the rays were thrown on to one of the inclined faces (10009) and sheltered from the other—still no motion was produced.

10014. The cube, being left hanging in the sun's rays, or in the shade, did not tend to change its position. The tendency apparent at first to a slow untwisting is now apparently all gone, perhaps because the weight of the cube has been hanging to it now 3 days.

10015. The suspending fibre is single and about 6 inches long. The time of a vibration to and fro by the torsion force of the filament is as nearly as may be 2 minutes.

10016. When a polarized ray is passed thrgh. an analyzer at an angle of 45° less or more to the first (either a Nicholl or a Tourmaline), and so part only of the light transmitted, that light is polarized in a new plane, i.e. in the polarizing plane of the analyzer. But what distinguishes one part of the light, that it should go through the analyzer with new properties, and the other not go through? How can that be, unless there were in the original polarized beam parts having relation, but still in different states to each other? Else how could one part of the beam be acted on in one way and another in another way?



10017. Is it that though vibrations are all in one plane, still some are more advanced in the course of the ray than others? That would, of course, be a difference in the parts of the ray: but then, cannot we arrange a beam or ray so that all the undulations shall be at the same moment of time the same distance from its origin?

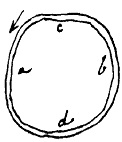
10018. *Gravity* (10082). Surely this force must be capable of an experimental relation to Electricity, Magnetism and the other forces, so as to bind it up with them in reciprocal action and equivalent effect. Consider for a moment how to set about touching this matter by facts and trial.

10019. What in Gravity answers to the dual or antithetical nature of the forms of force in Electricity and Magnetism? Perhaps the *to* and *fro*, that is, the ceeding to the force or approach of Gravitating bodies, and the effectual reversion of the force or separation of the bodies, quiescence being the neutral condition. Try the question experimentally on these grounds—then the following suppositions or suggestions arise.

10020. Bodies approaching by gravitation, and bodies separated per force, whilst gravitating towards each other, may shew in themselves or in surrounding matter or helices, *opposite currents* of electricity round the line of motion as an axis. But if not moving *to* or *from* each other, should produce no effect.

10021. Though two ordinary masses of matter may not do for want of power, yet a mass of matter subject to the gravitating force of the earth as the other mass, *may do*. Still, the manifestation of induced lateral effect can only be very small, if any.

10022. Then any motion *up* or *down* should do.



10023. No motion other than that caused by gravitation should do.

10024. So a revolving ring should produce the effect at *a* and *b* in opposite directions, but cause no effect at *c* and *d*.

10025. If any foundation for these expectations—then *falling* waters, etc., rising currents of air, smoke, etc. etc. should produce in nature equivalent effects.

10026. Then probably some connexion between such operations and the Negative state of the earth and positive state of the atmosphere and clouds.

10027. But in that case, would *not* be an effect of mere circumferential current, but somewhat resolved into the static condition of the electric forces.

10028. A falling body may either fall *through* a helix or *with it*. Can that make a difference in the results? The falling body may

either be the helix—or require to be independant of it. If the latter, it will shew an inductive action at right angles to Gravity.

10029. The weight of a body having a current purposely passed round it ought to be affected, i.e. if falling or rising, but not if still.

10030. But that a body should have less weight whilst falling and more weight whilst rising than when still or moving only horizontal, is a strange conclusion and against the general notion. Still, it may be true, for I do not see as yet that natural conditions contradict it.

10031. The test helix must probably be still and not moving up or down.

10032. After all, there is much which renders these expectations or similar ones hopeless: for surely, if founded, there must have been some manifestation of such a condition of the power in nature. On the other hand, what wonderful and manifest conditions of natural power have escaped observation, which have been made known to us in these days.

10033. Water falling down a pipe ought to give a constant effect.

10034. If the effect induced is in the falling body, and the current[s] formed are *in* its mass—then two bodies *falling* or *rising* side by side ought to repel each other. A falling and a rising body ought to attract each other. Could try this on strings or wire perhaps, but difficult unless in vacuos side by side.

10035. Effects produced on the bodies in the Universe.

10036. Effect in the revolution of a secondary round its primary, or of a planet round the sun—for there there is the *equivalent* of a fall by Gravity.

10037. The effect may be very small, even if it does exist.

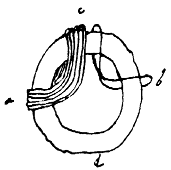
10038. If it exists, perhaps even then, it is the effort of gravity which at the moment of relaxation only produces the effect. Then an accellerated fall or rise would be required to produce a *continuous* effect of current or what ever else is produced.

10039. If so; stopping a body from falling should produce the counter effect—or from rising—and then an up and down motion would easily give an accumulative result, by properly adjusting the successive contrary effects.

10040. All this is a dream. Still, examine it by a few experiments. Nothing is too wonderful to be true, if it be consistent with the laws of nature, and in such things as these, experiment is the best test of such consistency.

23 MAR. 1849.

10041. Every revolving wheel—or rising or falling band—stream—etc. etc.—should then be a source of the effect or power.



10042. A wheel or ring might be good, thus, as giving a continuous effect of motion—for at *a* and *b* the results in contrary directions might be expected, but at *c* no inductive effect. The arrangement *a*, *c* would allow the radii of a wheel to pass: that *c*, *b* would not, but would require a ring only—and then a helix would be best.

10043. If any such effect as that dreamed of, then a body should fall slower or quicker close to a wall or upright rod of metal than in free space or vacuo, for the reaction of the induction would affect it in one case, and not so much in the other.

10044. The unchangeability (final) of Gravity not affected by any of these forerunning supposition[s], for the final effect when bodies at rest in relation to each other would be ever the same.

See Expt. Researches (1730) for a similar case.

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10045. If, as supposed above (10038), an effect is produced only in the conversion of power: then a continuous revolution would be of no use, because the equably falling parts would only sustain a state of tension, which would return and produce a counter effect when the fall was stopped. But then, should not the parts about *c* and *d* present some effect equivalent to stopping?

10046. Also, if due to the conversion of the passive to the moving state of a body travelling with or against gravity, then a pistol bullet in a barrel, fired upward or downwd., would be a good case for quick evolution of any lateral induction effect.

10047. For expts., let a constant stream of *water* or *mercury* pass through a tube within a helix.

10048. Can there be any induced effect *along* the direction of the falling body? Lead tube connected above and below by Vulcanized

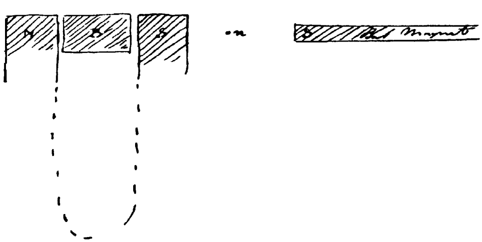
tube, and also above and below with delicate Galvanometer. Then let a good stream of water rush through it. Take care of temperature.

10049. A round Gutta Percha band—$\frac{1}{8}$th of an inch in diameter—was used to suspend a weight of 56 lbs.—the band stretched, but after some days the extension ceased and then the weight was well and freely sustained; and no signs of breaking or separation in the band appeared, even at the places where the knots were tied and where of course the band might be expected to cut. This is rather unexpected to me, considering that the band is so soft that when taken off from the support and weight, it could easily be bent at the same temperature into a distorted form, and also that the finger nail could cut it easily by drawing it across the band with pressure. The band had assumed something of a fibrous form and pieces an inch or two long could be separated by splitting or stripping.

10050. I cannot clearly understand the arrangement, described by Weber, intended to shew the polarity of bismuth under magnetic influence, as given in Taylor's Scientific Memoires, Vol. v. page 480. If accurate in the result, there must be something good there, though I doubt the evidence as to polarity. However, I made the arrangement this way—all the parts being in plan and therefore in the same horizontal plane*. N and S are the poles of a good horseshoe magnet able to raise 30 or 40 lbs.—B a block of bismuth; S′ a bar magnet at such a distance as to allow the small magnetic needle at *n* to stand in the same position as if both the horseshoe and bar magnet were away. B is a block of bismuth, and several blocks large and small were used. When a piece of iron was put in the place of B, it determines the power of S more towards N than when the iron was away, and consequently S is

* [10050]

or in view



weakened in action upon *n*, and the *s* pole of *n* approaches S. As I understand Weber, he says, that if B be bismuth, the little needle *n* is also affected, but in the contrary direction, as if the S pole had been strengthened by the added southness of the bismuth. I could obtain no effect of the kind. Bismuth seemed to me entirely *nil* in its action (10691).

28 MAR. 1849.

10051. If there should happen to be any result of the kind imagined, then a body moving *up* would produce *one* current, and moving *down*, the reverse current. Now these may be converted by a comutator into one consistent current and that may be sent through a Galvanometer for the time of a *half vibration* of the needle, and they by a *second* commutator be sent for the second half vibration in the contrary direction—then back in the first direction, and so on continually. This would seem to be a good way of accumulating the induced force or current, IF THERE BE ANY.

6TH APRIL 1849.

10052. Made certain preliminary experiments on the possibility of including Gravity force in the connected and convertible forces (   ), and for this purpose used a good hollow helix connected with my Galvanometer. The connexion was good, for the insertion of the end of my pocket knife into the helix caused motion of the Galvanometer needle through 10° or more. The vicinity of the stand which supported the helix (with its axis vertical) affected the needle so as to move and hold it 4° out of position; but still, that effect was permanent and did not trouble the results.

10053. Had a loose core of *copper*, about 4 inches long and $\frac{7}{8}$ of an inch thick, which could pass easily through the helix. Let this drop through the helix, and then taking it out below, let it drop through again, and so on several times together in rapid succession, but there was no effect upon the Galvanometer.

10054. Had a similar $\frac{7}{8}$ copper core 24 inches long and used this in the same manner. No action.

10055. Passed them *upwards* several times in succession—still no action.

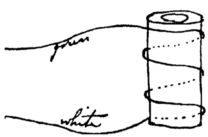
10056. Fixed the 4 inch core in the helix and let *both together* drop (with the helix axis vertical) about 2 feet—no action.
10057. Allowed the helix to drop without the helix core—and also raised it several feet—still no action.
10058. Used *Glass*, *Bismuth* and *Shell lac* cores in the same way, moving either *with* or *without* the helix *up* or *down*—but obtained no action.
10059. So here no encouragement—but as the effect of the descent may be counteracted and compensated by the effect of the stop, and both occur in a very short time, there may be mixture of the two and a nil effect. So must let the ascent and descent be thrgh. much greater distances, as from the ceiling to the floor of the lecture room. Can easily manage this with the *helix and core* or *helix alone*.

13 APRIL 1849.

10060. In reference to my Magneoptic effect and also Plucker's Magneoptic effect: it is a very striking thing to see the line of Magnetic force thus constantly perpendicular to the line of no action on a ray of light: or as we may in other words say it, constantly perpendicular to the undisturbed ray.

10061. I have been arranging certain experiments in reference to the notion that Gravity itself may be practically and directly related by experiment to the other powers of matter (10018), and this morning proceeded to make them. It was almost with a feeling of awe that I went to work, for if the hope should prove well founded, how great and mighty and sublime in its hitherto unchangeable character is the force I am trying to deal with, and how large may be the new domain of knowledge that may be opened up to the mind of man (10253, 10319).

10062. I have a hollow helix about 4 inches long and 2 inches external diameter, the internal opening being an inch in diameter. It contains about 350 feet of covered copper wire; the covering of the wire at one end of the helix is green, and at the other, white, and the direction of the spirals is as in the figure.

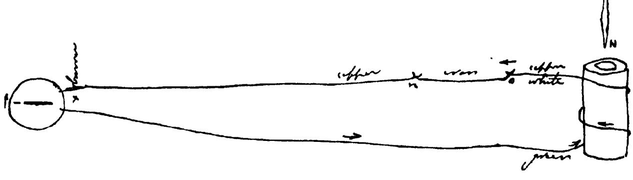


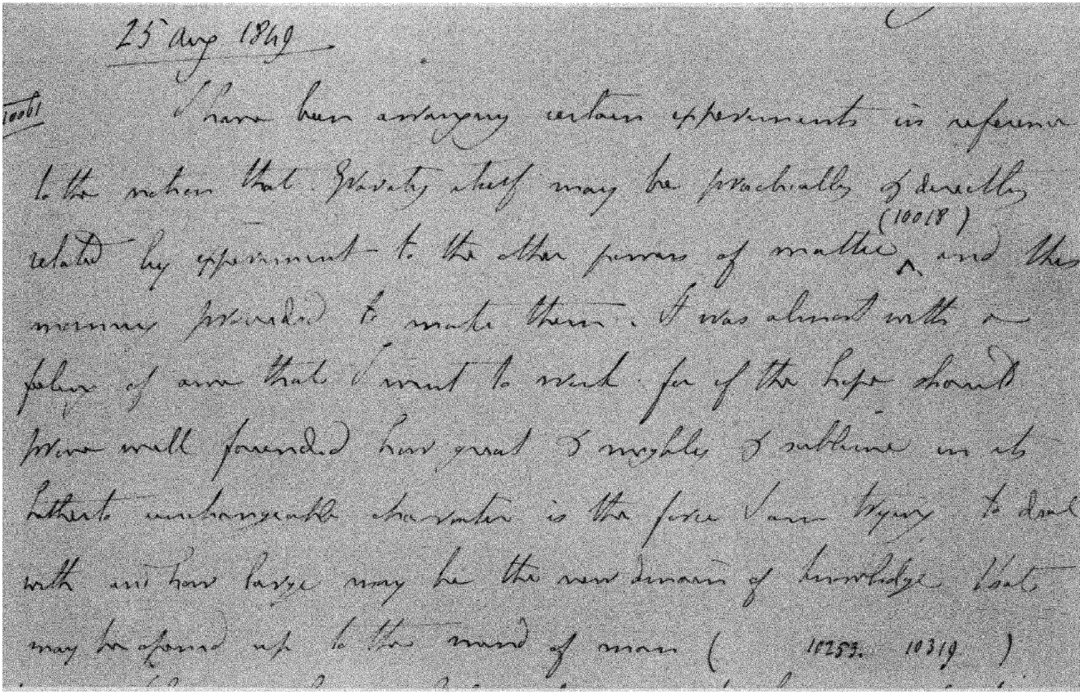
10063. I have different cores of copper, bismuth, iron, etc. which go into this helix, and having introduced the core of copper, which is 7 inches long and $\frac{3}{4}$ of an inch in diameter (weighing of a lb.), I wrapped the whole up in a thick cloth, and tied it up, leaving the ends of the Galvanometer wires out. This helix was then suspended by a cord passing over a pulley in the lecture room, by which means it could be raised through a height of 36 feet (about) and also allowed to fall thrgh. the same space, being retained the while in a given position by a slight friction on the running cord—and the mass itself received on cushions placed on the lecture table (in the lecture room) beneath.

10064. My delicate Galvanometer was placed on the seat under the clock and gallery, and two covered copper wires communicated from it to the helix, long enough to allow of the elevation and falling of the helix through the 36 feet and yet carrying any current generated to the Galvanometer. The good of the connection and also its direction was ascertained in the first instance by the use of a thermoelectric and also a magneto electric arrangement. Thus*:

10065. When the helix was placed vertical with the white end wire upwds. and connected with the Galvanometer (of course

* [10064]



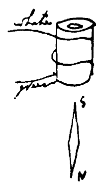

25 Aug 1849

10061 I have been arranging certain experiments in reference to the notion that Gravity itself may be practically & directly related by experiment to the other powers of matter (10018) and this morning proceeded to make them. It was almost with a feeling of awe that I went to work, for if the hope should prove well founded how great & mighty & sublime in its hitherto unchangeable character is the force I am trying to deal with and how large may be the new domain of knowledge that may be opened up to the mind of man (10253. 10319)

PAR. 10061, AUGUST 25, 1849, WRITTEN AS FARADAY BEGAN HIS UNSUCCESSFUL EXPERIMENTS TO SHOW A RELATION BETWEEN GRAVITY AND ELECTRICITY (*full size*)

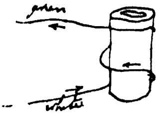
horizontal in plan) and also a piece of iron wire introduced at *n* and *o*, then the heat of a spirit lamp or even the fingers made the galvanometer move, and the direction of the hair end of the Galvanometer needle was to the right hand. In this case it was shewn that connexion was good and also that the current causing this deflexion of the needle was that indicated by the arrows.

10066. The iron wire was removed and the copper connexion completed, the position of the helix, Galvanometer, etc. still remaining the same. Now the N pole of a magnetic needle, which pointed downds., being introduced from above, made the hair end of Galvanometer needle turn to the *left*—but when the pole was lifted up and out, the needle turned to the right—giving in the last case a current in the same direction as that produced above by heat. This agrees with the known fact of direction, and shews that when the end of the needle passed to the right hand, the current was going round the helix as indicated by the arrows.



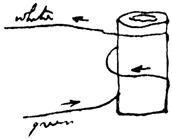
10067. When the needle was placed below the helix and then the helix dropped so that it should pass over the pole, then also the Galvr. needle went as before to the left, and on lowering the Mag. pole out of the helix passed as before to the right. This is as it should be, contrary poles entering at opposite ends giving def[l]ection in the same direction. But if the Magnetic lines of the earth could do any thing at all with a helix rising or falling among them, one would expect an effect like that of bringing the helix down over the S end of a needle. These experiments were made, the core of copper being of course away.

10068. Now used the helix with the core of copper, the green wire end being uppermost (10064) and the helix with its core vertical. It was made to ascend (by pulling the cord) 36 feet and also fall through as much, but the fall was by far the best motion, because it was smooth, continuous and accellerated until the cushions caught the helix when it suddenly stopped. I could not distinguish any certain effect either of the ascent or descent, and yet have a suspicion that as it descends, the hair end of the Galvanometer needle goes or tends to the left, and as it ascends perhaps has a tendency to the right. Now as the connexions were never changed either at the Galvanometer or the wire, this would shew a tendency to the formation of a current *during the descent*

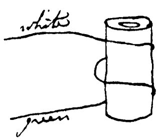


the reverse (in the wire) of that produced by the Electro thermic effect (10065) or the same as that figured in the margin.

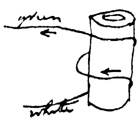
10069. The helix with its copper core was now inverted, so that the white wire end was uppermost. Now, on ascending, there was nothing clear, though perhaps a trace of needle movement to the left hand—but on descending am almost sure there was motion about the $\frac{1}{4}$ of a degree to the *right hand*—stopping at once as the descent of the core and helix was stopped. This is contrary to the direction of the former motions (if they were real); but still, that should be the case, for now the current ought to be reversed in the Galvanometer wires if due either to Gravity or to terrestrial magnetism, that it may be *the same* in the helix in relation to its position in space, or rather in respect of the lines of Gravitating force. This is easily seen by the diagram and gives great encouragement—but I must very carefully verify the effect and discriminate the true cause.



10070. Now removed the copper core and used the helix alone with the white end wire uppermost. The result was very doubtful, but if there was any effect, it was the same in kind as in the last experiments, i.e. as if the core were there. I think there was effect but am doubtful. It may well be that there should be effect, for one part of the coil may act as core to the other parts, just as a current of Electricity in a wire, when stopped, induces in the carrying wire exactly as it can do in a neighbouring wire.



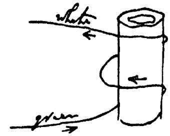
10071. Reversed the position of the helix so as to have the green end of wire uppermost, but no core. Very little action, if any. But on using stronger spectacles, think I can distinguish an effect. The galvanometer needle seems to go to the *right* whilst the helix is descending, and to the *left* at stopping, and the effect is rather plain, unless I deceive myself. This would still indicate the same undeviating and consistent effect.



10072. When using glasses, it is very necessary to be careful of any magnetic effect they may produce: but being aware of the effect, it is easy by perfect stillness at the galvanometer to avoid error.

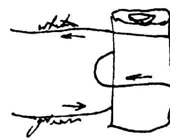
10073. I now changed the ends and placed the helix as before (   )—there is a trace of effect—less clear than in the last position (10071). The galvanometer needle was deviated to the

left during *descent*, as figured in the margin, where the current is recorded by the arrow—and was to the right at the stopping. All this is consistent.



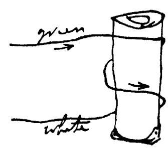
10074. Put a *bismuth core* into the helix: it was about $3\frac{3}{4}$ inches long and about $\frac{3}{4}$ of an inch in diameter. When the white wire end was uppermost, there was no clear effect on the ascending of the helix and core—but on descending, a little effect, the end of the Galvanometer needle moving a little to the right at the end of the descent. The helix falls with a sort of blow on the cushion. I think it is not possible that this blow on the table can mechanically cause the effect on the needle.

10075. Reversed the helix and bismuth, so that the green end upwards; there were traces of motion on the Galvanometer needle to the *left* at the end of the descent, and then they stop, as the motion of the helix stops. It is not to the right as before, but the blow on the table is as before and therefore that can not cause the effect in a mechanical manner. The two effects are consistent and as, when the helix has white end wire uppermost, the Galvr. needle went to the right at the end of the descent, so the current if it existed must have been as in the figure, and consistent with all the former results.



10076. It is to be remembered that the core can probably shift a little at the moment of the fall on the cushion. Perhaps the motion of the core *through* the helix may be an important condition—perhaps not. If there is any induction of an electric current, it can only be in the vicinity of the rising or falling matter.

10077. Now employed an *iron* core in the helix; it also was $3\frac{3}{4}$ inches long and $\frac{3}{4}$ of an inch in diameter, being the size of the bismuth core—the green end wire of helix was [uppermost]. The effect was I think produced, but as little as in any former case. As helix descends, the needle motion, if any, was to the right, or as marked in the diagram—it was to the left at stopping.



10078. I reversed the helix and core, so that the white end was upwards. The effect was very trifling, but I think needle end went to the left as the helix descended and then at stopping of the latter went to the right.

10079. Now these results with iron are consistent with each other—but give *opposite directions* to the former results. They must

therefore be carefully repeated. If true, they will perhaps shew two things. First, that the magnetic effect of an iron core is very small indeed, and cannot produce or account for the former results. Next, that if Iron produced any magnetic effect, it was probably an effect in the contrary direction to that produced by Gravitating force.

10080. As to Magnetic action. I do not see how the helix moving (in falling or rising) amongst magnetic lines of equal force and parallel in direction, can have any magnelectric currents produced in it. To produce these currents, the moving wire must cross the lines of magnetic force, but from the constitution of the helix and the parallelism of its fall, for as much wire as crosses the lines of force in one direction there is an equal quantity crossing them in the other direction, and so no current can be produced. If a ring of metal be moved in any direction through or across lines of magnetic force of equal intensity, no current can be formed in it provided it be kept parallel to itself. If it revolve about a diameter, the case alters, but that is not the case of the helix.

10081. I do not see how an iron core can alter the effect, unless indeed it receed from or approach to some magnetic beam or column in the building, and then its own magnetism can be increased or diminished, and it will of course produce currents by induction at these times.

10082. If there should be any effect of Gravity convertible into Electricity (10018)–then we might perhaps find delicate means of shewing the gravitation of one body to another, and verify Cavendish's and Baily's conclusions, by comparing the action of the earth with the action of a heavy ball of lead. For the motion of the moving core (representing a gravitating body) in a horizontal instead of a vertical direction would entirely eliminate the action of the earth, and leave the bodies in the direction of the axis of motion to act on each other alone.

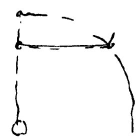
10083. Perhaps in that way a convenient and measurable test and indication of gravitating action of different bodies might be contrived and usefully applied.

10084. Then, under such suppositions, would the centers of gravity of two bodies, as two balls, when acted on by *the earth*, be also the centers when they were acting on each other i.e.

considering their size and shapes? It could not well be if the shapes were extended or irregular, i.e. the center of gravity of a globe or of a solid ring of equal weight would be the same for the earth's action, but would they be the same to a *near small* globe in every position of the ring?

10085. If Gravity has relation or connexion (as I suppose) with electric forces, and therefore with magnetism; then it is possible that the rotation of the earth on its axis, by its counteracting gravitation in the equatorial direction, may have *something* to do with terrestrial magnetism and the direction of its polarity. And if so, then the rotation of a globe of copper ought to develope some magnetic effect in the direction of the axis of rotation. It should be rotated on an axis parallel to the *dip*, or else inductive magnelectric currents would be produced. In any case, the effect expected can only be small, and therefore interfering sources of error must be carefully excluded, and the amount of action magnified as much as possible. Astatic needles with long intermediate fibres or rods and carefully sheltered, on fixed supports, would probably be required.

10086. But if gravitating force is convertible only whilst motion is being *acquired* or *lost*, then the mere rotation of a body and its tendency to expand at the equator should do *nothing* unless it really expands or moves. Unless indeed some other consideration is developed, like that of the moon in its orbit round the earth, where whilst moving from *a* to *b*, it may be considered as *falling* from *a* to *c*.



10087. From the experiments (10067, etc.), it would seem just possible that there is an effect and that a current of Electricity is induced round the line of gravitating force joining two bodies together, when one of the bodies moved thrgh. the active exertion of that force, and that the law of force may be as follows. If a man were falling with his face towards the earth, the electric current which would tend to form round him would be in the same direction as the hands of a watch which he should be looking at, or screw fashion. If he were to retreat from the earth, in the contrary direction. So that if he were to move face forward either to or from the earth along the line of gravity, his hands, moved as the hands of a watch or thus, would indicate the direction of the induced current.

10088. Suppose all this. Then, if two bodies gravitate towards each other, they ought not to induce electric currents whilst *still* in respect of each other, but if they approach each other they should. Now the direction of the rotation round them cannot be the same for both, according to the above law, but in contrary directions, for the two are moving in contrary directions.

10089. If we consider the magnetical result of two such induced currents, it would give for the two bodies such polarity that like poles would be towards each other.

10090. The induction must, I think, if it exists at all, be round the line of gravitating force.

10091. If of the two bodies only one moved, then would that one only produce rotative current round it by induction? If so, of course either may have, as above (10087), its own current with its own direction.

10092. But is it likely that the moving body should not, whilst moving under these suppositions, induce a current round itself, and yet not affect the state of the gravitating power in the *still* body—since the gravitation force of that quiescent body is in part the cause of its motion? Would look like a power of affecting one end of a line of gravitating force and not the other. This not likely and so is against all my suppositions, but we shall see how experiment testifies, and whether it only modifies some of my deductions and conclusions or sweeps them away altogether. Which may well be.

10093. Suppose that a body, in falling to the earth through a helix, induces in that helix a *given* current of Electricity.

10094. Then if we send before hand such a current through the helix, that current ought to affect the fall of the body, retarding it? If we send the contrary current, it ought to facilitate (?) the fall—or the reverse? A mutual effect ought to result.

10095. The converse ought to be the case for ascent.

10096. But if the gravitating body be QUIESCENT, will a current one way or the other in the helix make it tend to ascend or descend—or must it be *moving* subject to gravity before it can be thus affected?

10097. If, being quiescent, it could be affected, the result would be a production or generation of *gravitating force*, and that is hardly possible.

10098. If a *moving body* were to rise or fall with *different momenta* under the condition and influence of a surrounding current, it would be equivalent to a generation of gravitating force of a similar kind.

10099. If an electric current could affect bodies *moving* in the line of gravitating force: then what would or could it do with a body moving at right angles to this line? Surely not affect it—or if affecting it, how?

10100. A body falling through a thick copper tube ought to fall easier than if the tube were away—and also cause currents in it and so make it even for the time magnetic. Could it affect an astatic magnetic needle?

10101. The falling body may induce currents in itself—and the induced and separated current may be a very small part of that actually set in motion at the time.

10102. Perhaps more might be separated by particular cores used as the falling body—as insulating cores of glass or shell lac, or cores in pieces—cores of covered copper wire—of copper filings in a glass tube—of water—Sulphate of baryta—Iron—Gold—Silver.

10103. Perhaps as a core a solid helix of covered copper wire—soaked in solution of shell lac and dried well—might be the best; for the particles of the wire may act as core to the whole helix.

10104. If suppositions are true, then could not a falling river or a falling stream in a tube be converted into a Gravelectric arrangement, taking advantage of the places where the motion comes on and goes off? Now as water passes thrgh. a pipe of this shape—motion comes on from *a* to *b* and goes off from *b* to *c*. I put down all these fancies as I write, to stir up all the points of analogies, contradictions, consequences and absurdities, that I may the sooner both by *experiment* and reasoning settle what ground of expectation there may be in this matter, and work out the thought to an end. But the more I write the less I expect.

10105. Motion of a body *across* the lines of gravity should produce no result.

10106. Motion downwds. in line of gravity should give a result.

10107. Motion upward in line of gravity should give a contrary result.

10108. Motion downwd. in line of gravity *faster* than if the body were falling by gravity alone, should (I think) give more effect in a given time than if falling by gravity alone: should in fact give an effect proportionate to the motion in a given time.

10109. If so, might produce the great effect of much fall in a *short space* by quickness of motion. Have set on for construction a trial apparatus on this principle (10133).

10110. Then what would be the effect of a shot from the moment of quiescence up to the moment of obtaining its maximum velocity–or again, at the moment of loosing its velocity by being stopped? Could perhaps try a pistol or even a cannon shot.

10111. As to influence of lines of terrestrial magnetic force: one might neutralize these by surrounding the falling body by electro or common magnets, so as counteract or alter the direction of the lines of force. This should not affect the gravity action.

10112. If there should be any truth in these vague expectations of the relation of Gravitating force, then it seems hardly possible but that there must be some extraordinary results to come out in relation to celestial mechanics–as between the earth and the moon, or the Sun and the planets, or in the great space between all gravitating bodies. Then indeed, Milton's expression of the Sun's magnetic ray would have a real meaning in addition to its poetical one. How would the compensation of centripetal force by the revolution of a secondary in its orbit work out?

10113. Supposing a Gravelectric current could be obtained, we should have to shew by it the production of

the electric spark,
„ magnetic effect,
„ decomposing or electrolytic effect,
„ heating effect on a wire,
„ convulsions of a frog, etc. etc.,

and there would probably be no difficulty in arranging so as to produce these results. The only point is, does the relation with Gravity exist as a root on which to build up these matters?

10114. Suppose a thick hollow copper tube falling vertically. If it can induce a current on a helix outside of it, can it also induce a current in a helix inside of it, and in what direction should that current be? This is a serious question, affecting the whole

expectation. The result, if not entirely negative, would give many variations of condition as regarding bodies passing up and down, in and by each other—of great importance.

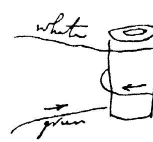
30 AUG. 1849.

10115*. Arranged matters in lecture room generally as before (10064), but am not quite sure that contacts are the same in direction. However, to try that and to see that all was right, a piece of iron wire was made fast to the green end of the galvanometer wire, and when that junction *a* was made warm by the hand, the cold end at *b* was brought into contact with the copper end at *c*, and immediately the hair end of Galvanometer needle moved to the *left*; thus shewing the goodness of the circuit, and also, as the current at *a* was from hot copper to hot iron and therefore as marked by the arrow, so it shews that the connexions at the Galvanometer were the reverse of those on the former day.

10116. I now removed the iron wire and completed the circuit by copper contact of *a* and *c*. Found now that no warming by the hand or otherwise of the contact at *a* or at *d* had any effect on the galvanometer—all being copper there. Also that no blowing on *a* or *c*, or the Helix generally, had any effect on the galvanometer. So that no result of warming or cooling or wind by letting the helix rise or fall through the air 30 feet could be the cause of any effect at the Galvanometer.

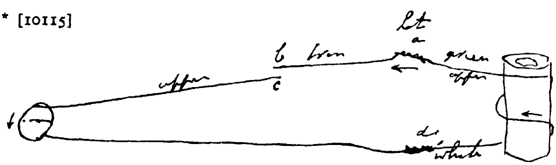
10117. The Galvanometer was made more delicate by placing a magnet opposite to it, so as to counteract the earth's action and increase the time of a vibration from 3″ to 6″, 7″ or 8″.

10118. The large copper core (10063) as before was put into the helix and tied up, and then ascents and descents examined. When the *white wire end* was uppermost—ascending cause[d] the hair end of the needle to go to the *left*—and descending caused it to go to the right. So descent caused a current as marked. This happened again and again.



10119. *Green end wire* uppermost. Ascending—the galvanometer either not affected at all or a little to the left; descending—no sensible motion of the needle. Three or four times over—very uncertain.

* [10115]



10120. Again, white end of helix upwards. Same as before (10118). Whilst ascending—needle to the left; and whilst descending—needle to the right—but *less* effect than during ascent.

10121. So the needle goes to the left whilst the helix is ascending, whichever end of the latter is uppermost. This is not like an effect of gravity, for to agree with a cause of that nature, the current ought to be inverted.

10122. When the helix and core was horizontal, there was only very slight and uncertain motion at the Galvanometer, either by ascent or descent.

10123. *Green end of helix* upwards—no action at galvanometer either at ascent or descent. But as one of the wires came off at the last, it is just possible that the contact was not then perfect. Still, it shews that no mere mechanical shake of the wires affects the galvanometer, but that an electric current is produced by one cause or another.

10124. Perfected contacts and made helix green end upper: I am sure there is an effect of ascent and descent, but not quite sure of the direction—think the needle goes to the *right* whilst helix ascending and to the *left* whilst descending. Perhaps the effect of stopping coming on may confuse the results.

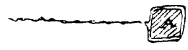
10125. *White end of helix up.* Effect distinct in its occurrence and I think, during ascent, needle is to the right—during descent, to the left. Again, *white end up*: now during descent, to the right, and during ascent, to the left. Very contradictory.

10126. *Green end up.* Very little if any action. If any, appears to be to the right during ascent and to the left during descent.

10127. Now made ascent about 20 feet, and as the needle swung back from its first impulse—made descent—and then again ascent—so as to add the effect of several ascents and descents together on the needle. There certainly is a swing from one cause or another, but the effect is small: the swing appears to be to the right for ascent and to the left for descent.

10128. Same thing with the white end of helix up. First time, it seemed to be to the right for ascent and to the left for descent—but upon continuing the action, no further effect.

10129*. Thought it possible that the mere loop of the Galvanometer wires falling or rising might cause the electric current by cutting the magnetic curves of the earth, for as the helix rises or falls between *n* and *o*, it is likely enough that the galvanometer wire will hang in loops, as from *m* to *n*, and then in rising or falling the different parts of this loop may not pass through an equal space across the magnetic lines of force of the earth. So the Galvanometer wires were twisted together from the Galvanometer to within 6 inches of the ends, that the corresponding parts of the two wires might equal[ly] cut magnetic lines of force in falling and rising, and then, being connected, the extreme end of the loop was made to pass round a brick (instead of being attached to the helix), as seen in this plan; the brick being raised or lowered in place of the helix, and with either the face A or the opposite face B uppermost.

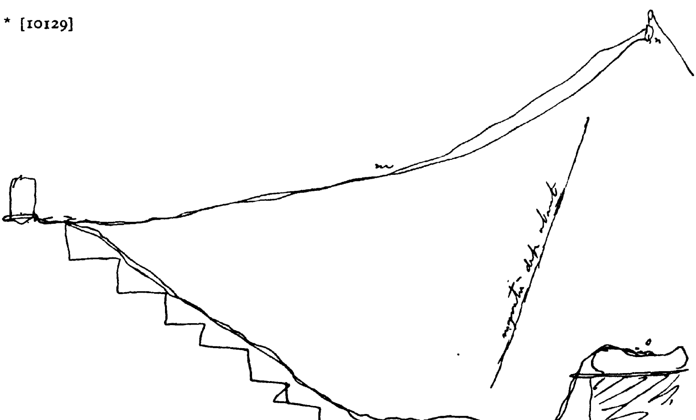


10130. And now there was *no effect* at the galvanometer either by ascent or descent—or with face A or B upwards. Though having tried hastily with the brick before twisting the wires, I found there was then a little effect.

10131. Replaced the helix and copper core at the end of the twisted galvanometer wires (10129); but with either the white or the green end up, there was *no effect*, though the circuit was found by trial at the Galvanometer *perfect*.

10132. So now I believe that all the effect[s] I had heretofore obtained were due to the falling or rising loop of wire (10129) and not to any effect of Gravity. At all events, we are purifying the inquiry from interfering causes.

* [10129]



10133. If, then, a falling helix or a falling body in a falling helix can produce any effect *by Gravity*, it must be very small in amount. But perhaps if a body were falling or rising in a *helix* AT REST, the effect may be different and essentially so (10095). Hence have this case to examine by the apparatus which Newman is making for me.

Considerations.

10134. If many particles fall and each have an electric current induced round it, the result should be a current round the outside of the whole, but *none* on the inside, for there they neutralize each other.

10135. So, if many fall in a ring form, there might perhaps be a current or a tendency to a current round the whole on the outside—but none inside the ring.

10136. Then if a *copper cylinder* were to fall, having a helix *inside* and another *outside*, it might induce a current in the *outer one* but not in the *inner one*.

10137. But then, should not a thin cylinder in falling produce as strong a current as a solid core? This is not likely: the solid core ought to produce *more* or else NONE.

10138. Also then, how could a solid helix produce any resulting current?

10139. Yet—is not the state supposed above (10134) the analogue of Ampère's theoretical magnet, which may have resulting currents either round the *particles* or the *masses*? He says results are in both cases the same.

10140. Also, when a helix carries a current, an iron core inside is affected, an iron tube outside not at all. Which shews the great difference *within* and *without*. Something of a like nature may occur in gravitation effects. So go on experimenting with the apparatus (10133).

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10141. Have received the machine (10133) from Newman and worked with it, obtaining some results—but as it must go to be altered in some points, leave the description and notes for the present. I think it will serve its purpose.

10142. Have prepared some lead, tin and copper for experiments on the question whether the pure metals can be magnetic under one degree of force and diamagnetic under another.

10143. Have taken to pieces a fine old Zinc tree, i.e. a metallic crystallization of lead obtained by suspending a piece of zinc in a solution of the acetate of lead. Selected the lead, putting together the bottom portion consisting of large crystals, foliated—also the top part or that near the zinc, and the middle portion in two parts. Washed these in distilled water—pressed them and dried them. Whilst drying in the air, they heated by combination with oxygen and much oxide was formed.

10144. Tried to reduce and melt a part of the first portion in a German glass tube whilst passing a stream of hydrogen over it, but the effect was not good. It either wanted so much heat as to affect the glass or else the oxide was not reduced and the metal did not run together.

10145. Tried to reduce some by melting it with a little cyanide of potassium, but this also required too much heat.

10146. Put the lead therefore into a thin glass tube (german glass) with a few drops of oil of turpentine—or wax—or olive oil, so as to exclude air by its vapour; and when heated—shook the tube and stirred the contents with a clean splinter of wood and in this way made the metal cohere, running together, and finally obtained in each operation a good button of very soft crystallizable metal and a quantity of lead ash. The lead ashes of 1, 2 and 3, which were all melted in this way, were added to No. 4, and then that portion, with a little lamp black, mixed with a flux of equivalents of Carbs. Pot. and Soda, and melted in a crucible of white Cornish clay. The leads so obtained were afterwards, by refusion, pouring and the use of bronze chisels, shaped into pieces fit for future experiments. The following are the numbers and distinctions:

10147. *No.* 1. *Zinc tree lead*, the lowest part, supposed to be the best (10143).

10148. *No.* 2. *Zinc tree lead*, middle part (10143).

10149. *No.* 3. *Zinc tree lead*, middle part (10143).

10150. *No.* 4. *Zinc tree lead*—upper part near the zinc, and ashes

of Nos. 1, 2 and 3 fuzed in contact with lamp black and Carbs. pot. and Soda in a crucible (10143).

10151. Put on some reducing processes, using a Grove's battery of 3 pair of plates, and introducing 3 places of decomposition—for Lead, Tin and copper. As the lead and tin crystals formed, they were put back in the solution or separated untill enough was collected of each. The whole process of reduction went on for 12 or 18 hours or more.

10152. *No.* 5. *Battery lead.* A solution of pure Nitrate of lead was used and both the electrodes were of platinum, carefully cleaned, the positive electrode being large. Beautiful crystals of lead were formed on the Negative side, and a thick crust of per oxide of lead on the positive side. I had put some alcohol into the solution, hoping it would help the reduction, but I think it did nothing that way. The solution became more acid as the lead was separated from it, but abundance was used. The lead crystals, being well washed in distilled water—dried and heated in a tube with a drop of oil of turpentine, gave a good piece of lead, and some ash which was put into No. 4. This lead is No. 5.

10153. *No. 6. Is the Per oxide of lead* from the above process (10152) washed and dried.

10154. *Tin. No.* 7. *Battery Tin* (10151). The solution was of a good proto muriate. The negative electrode was platina—but the positive electrode was a piece of ordinary tin—so that no care was taken as to the special purity of the tin and solution used, but the process of reduction at the Negative pole altogether depended upon. The tin crystals were of course easily washed, dried and fuzed without any oil of turpentine, and made a piece called *No.* 7.

10155. *Copper. No.* 8. *Battery copper.* The deposited metal on the platina plate used as the Negative electrode—the positive Electrode being a piece of common copper and the solution common sulphate of copper.

10156. May in the use of the Machine (10141, 10162) easily distinguish Ferro electric, Bismuthelectric and Gravelectric results from each other—if they should all present themselves. Suppose an iron core moving vertically gives a current: then by putting ordinary magnets into place, we can reverse the direction of the

lines of magnetic force without altering the place of the core, and it will then give the reverse current. If bismuth in the same position gives a current by the earth's force, it will be contrary to that produced by the iron, and will also be reversible by reversing the direction of the lines of magnetic force. If a copper core give neither ferro or bismuth effect, but an effect of gravity, then that effect will *not* be reversed by reversing the direction of the lines of force.

10157. A further distinction would be in this—a gravitating effect would not be produced by copper moving in the horizontal direction, though magnetic curves might be passing in that direction, but the magnetic and diamagnetic effects would. So by comparing Iron, bismuth and (say) copper in the horizontal and vertical directions, the results might be eliminated.

10158. If the machine (10164) be competent to evolve the diamagnetic effects of bismuth—then probably also competent to evolve the effects of cores made up [of] crystals—or the magnecrystallic effect. Crystals of Calc. spar—red ferro pruss. potassa—bismuth, etc. might do. Sulphate of iron also very good probably. Such cores might be used horizontally, the magnecrystallic axis being in one direction or the other.

10159. They would however require probably to be thrown into condition by external magnetic or electric forces, and might, in that view, be subjected to *fixed magnets*, *poles and a fixed helix*; or to a double oscillating helix, or a double fixed helix, one of the helices carrying an original current—this alone without a core would not affect the Galvanometer.

10160. Perhaps have a paper such as the following: *Contributions to Magneto-electric science*—containing results relating to GRAVITY—*Polarity of bismuth when diamagnetic*—*Magnecrystallic induction*—*Condition of pure lead or tin as magnetic and also diamagnetic*.

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10161. Du Bois-Reymond of Berlin has an experiment on the evolution of electricity by muscular action, which consists in connecting the wire ends of a very delicate galvanometer with two basins of brine—putting the hands into these basins and then rendering the one or the other arm rigid. A current is said to flow

through the two arms and galvanometer, and is in direction reversed as the one or the other arm is made rigid. Tried to repeat the experiment with my galvanometer, but found I had not time or means at hand to eliminate and remove all the disturbing actions, of which there are many in the hands, basins and electrodes. Think that perhaps two large tubs or vessels of water would be better than two small basins.

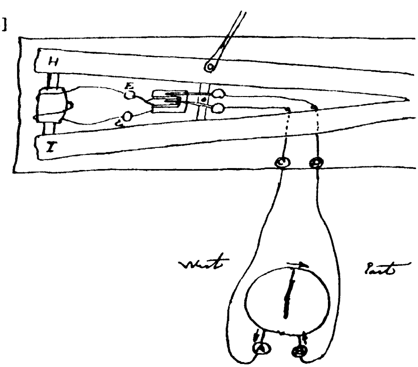
10162*. Have marked the screws of the Galvanometer and of the machine (10164) by letters, so as to secure position and record it correctly. When a current was sent through the galvanometer by a voltaic pair, it deflected the end of the needle to the east as represented, when the current entered at B and came out at A.

10163. When the helix was in and all was connected except at G, a piece of iron wire held in the warm fingers against the end of the copper helix wire and its cold end against the screw at G, caused the needle end to go West; so shewing that the communication for a feeble current was complete and also, as current would be from iron to copper when both were warm, that the position of the commutator was as represented.

10164. Now this machine is so constructed that by turning a wheel the double [illegible] arm H I can be made to vibrate thrgh. about 3 inches—and as it carries a core between H and I which can be taken out and changed at pleasure, this core also vibrates or moves to and fro in any position in which the machine is placed. There is a *fixed* helix about the core containing 228 feet of fine copper wire, and the direction of the helix is as is represented in the figure (10162, 10169, 10189).

10165. There is also a commutator which, by an adjustible pin, changes the positions of the wires as the core moves to and fro,

* [10162]

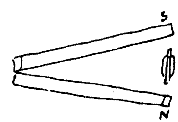


so as to throw into a nearly continuous stream the alternate currents of electricity which may be formed as the core advances one way or the other. This was arranged so as to change the connection a little before the motion in the one or other direction was stopped —so that all the effect of stoppage in the one direction was added on to the effect of coming on in the other direction.

10166. An *iron core* was put into the instrument (10187). Then on working the instrument a deflection was obtained at the Galvanometer. As the needle swung back, the working was intermitted, and renewed as it returned in the first direction. The end of the needle went E. as the machine was worked and therefore the current entered the machine at screw C and left it at screw D whilst the motion was going on.

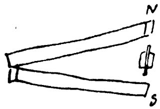
10167. Whether the iron core (with the machine) was horizontal or vertical or inverted vertical—or whatever the position of the machine, the result was the same both in formation of a current and in its direction. Hence it was evidently independant of the earth's magnetism or of gravity, and dependant on the core and probably its magnetism—for in all these trials the same connexions were preserved.

10168. The machine was now placed at a distance from the Galvanometer, but with the same connexions as before ( ). It was horizontal and two bar magnets were so placed that their ends were at the end of the range of motion of the iron core, so as to make it subject to their magnetism and be itself a magnet in consequence. Then the machine was worked (10164), and immediately the galvanometer well affected, and as the needle end went as before to the East whilst the machine was worked, it shewed that the iron was magnetic, as before, being strengthened in the same direction.



10169. Now the connexions are such that D and E (10165) are connected by the commutator during the rise of the iron core (when in the upright first position (10167)), until just before the stopping of it, and D with G during the fall of the iron until just before its stopping; whether the machine is turned the one way or the other. But as the current is out of the machine at D and in at C all the time of working, therefore the current is from C to G or the bottom of the helix, and round it to

the top, as the iron rizes—and in the reverse direction as the iron descends.



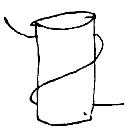
10170. Then the magnet ends were reversed so as to be in the direction represented, the core being horizontal and every thing else unaltered; and now the end of the Galvanometer needle went West as the machine was worked, thus shewing change in the direction of the induced current and confirmation of the former results with the core.

10171. Hence the magnetic effect of iron is clear enough in this machine and can easily be distinguished from other effects.

10172. The iron core was taken out and examined by a magnetic needle; it was very soft and did not present any trace of magnetism by an ordinary magnetic needle—so that a very small amount of magnetic force can in this way be detected.

10173. The machine minus the core but with the helix and magnets in their place was worked, but gave no indications at the galvanometer. This is as it should be, for the magnet and helix were all stationary, but it shews that there is no source of error there.

10174. Put the copper core (10187) into the machine in a horizontal position and kept the magnetic poles there (10170) but could obtain no effect at the Galvanometer; reversed the magnetic poles—still nothing. No signs of diamagnetic effect here or of any other: is as if copper were indifferent in the horizontal plane at least.



10175. Placed the machine with copper core vertical (no magnets). There was no sensible effect—or if an effect, it is very small indeed—thought there was motion of the needle to the E. whilst working—which would resolve itself into a current upwards through the helix whilst copper rising and downward thrgh. it whilst copper falling.

10176. Arranged a candle light at the Galvanometer and also a reading lens; the latter of much use.

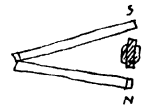
10177. Repeated the copper expt. (10175)—very little effect if any—but if any, needle is to the East whilst machine at work, and therefore current upwds. round helix as copper ascends and downward as it descends (is as before (10175)).

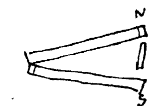
10178. Turned the machine upside down to invert the copper

core—but could get no clear effect—no clear reversion of the supposed induced current.

10179. *Bismuth core* (10187). Employed a bismuth core horizontal and without magnets—no effect.

10180. Placed magnets thus; on working the machine the end of the galvanometer needle went slightly but distinctly East. Reversed the magnets so as to change the poles. On working the machine there was a little motion of the needle, but it seemed in the same direction as before and not reversed—but the effect was very imperfect. On a repe[ti]tion of the last experiment, did seem to get some effect, the needle end passing to the West as the machine worked.

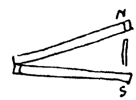




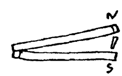
10181. These directions and effects appear to be the *same* as those with the iron in the former experiments (10169).

10182. Arranged the Galvanometer ends so that I could reverse the direction there as the needle swung to the right and left, and so keep the machine continually working and give an impulse to the needle *both ways*.

10183. The bismuth core and Magnets being in the machine thus, the Galvanometer needle was well deflected, the needle end going W. whilst C was connected with A and D with B. This agrees with former bismuth results.



10184. Made the poles thus*, and obtained a deflection, but it was as in the last case, shewing some error.



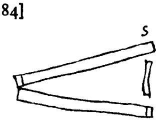
10185. Again, with this arrangement, and now end of needle went to the East with C and A connected, and so contradictory (10183).

10186. Again, with†, and now the needle end to the West with C and A connected. All very contradictory.

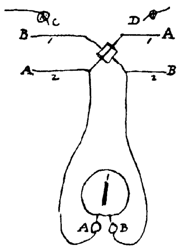
10187. The size of the cores of iron, copper and bismuth is $5\frac{1}{2}$ inches long and $\frac{3}{4}$ of an inch in diameter.

10188. Took off the helix and connected E and G by a short copper wire; then worked the machine, and got no effect, so that no current is caused by any heat of friction or other circumstance at the Commutator.

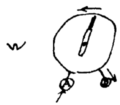
* [10184]



† [10186]



10189. I have had the wire in the helix of the machine increased to 516 feet in length—the direction being of course as before*. I have also cut away the wood of the arms of the frame†, so as to be able to bring magnetic poles nearer up to the ends of the core in its vibrating course, and so have a stronger effect on iron or bismuth. I have also a new arrangement of the wires at the galvanometer of the kind figured, so that by fixing the ends of the wires C, D coming from the machine to corks, and holding these either in contact at 1, 1 or at 2, 2, the direction of the constant current coming from the machine might be changed in its passage through the galvanometer according as the needle was swinging one way or the other, and so increase the effect there. This answers very well.



10190. Fixed the *iron core* in the frame (10164), tried the connexions and found them all perfect. Also ascertained the direction of the current in the Galvanometer and found it to be thus: i.e. when current in at A and out at B, the hair end of the needle went to the west.

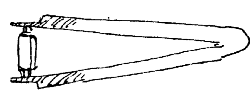
10191. On placing the Magnetic bars as before (10168) and then working the machine, the Galvanometer and its needle was much affected. On reversing the Magnet bar ends, the effect was as great as before but in the contrary direction. On tracing the connexion and position of the parts, they were found to be as below‡ at the time that the core was moving from S towards N; and as the end of the needle then swung to the West, it shewed that a current was going through the wires in the direction of the arrows.

10192. The magnetic action of iron is therefore well shewn and the direction of the current which it induces now well ascertained.

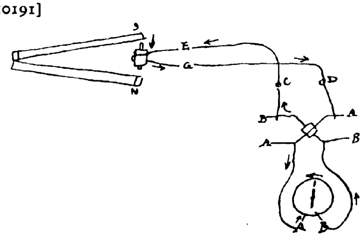
10193. The *bismuth core* was put in the place of the iron core,

* [10189]

† [10189]



‡ [10191]



all things else remaining the same. On working the machine, the Galvanometer was again affected, though not so strongly as before, but the direction of the induced current was the *same* as for iron (10191).

10194. Removed the bismuth core and left all other things unchanged; now worked the machine and found a feeble deflection as with bismuth and in the same direction as before, so that the former effect was not due to the bismuth (10193) but is some effect of iron.

10195. Thinking it might possibly be due to the steel springs which are used to receive the momentum of the stopping core ( ), the ends of which approach to and receed from the helix as the core and the carrying arms pass to and from, I took them out and so left the machine without a core or the springs but with the magnets in place. Then using it, there was no effect; but on reversing the magnetic poles, there was an effect at the Galvanometer, small, but the same in direction as that of iron.

10196. On removing the magnets there was no effect—so that the cause is not in any other part of the machine. On restoring them, the effect was produced again.

10197. There was a convolution of the loose part of the helix wire and I thought that might produce the effect, but on removing the convolution the effect continued.

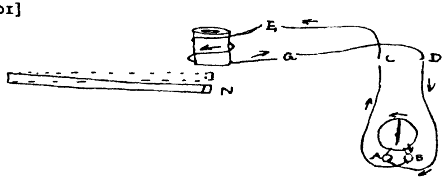
10198. I found that the mere approximation of one of the magnetic poles towards the fixed helix, if only for half an inch, made a swing of 2° or 3° of the Galvanometer needle (10201); and as the whole table of support shakes and the shake is synchronous with the motion of the commutator at E, G, I have little doubt that that is the cause of the effect, for the trembling may be a continual approach and recession of the magnet poles and helix, which by the commutator produces a continuous electric current.

10199. To remedy this, should have the machine and magnets well fixed on stone and strutted or keyed together.

10200. This effect can only interfere with cases where the magnets are in use, and not when they are absent.

10201*. In order to obtain the direction of the current induced by the approached magnet (10198), the connexions were made as in the figure, and then the magnet moved into the dotted position,

* [10201]



i.e. approximated; this sufficed to send the end of the needle to the West, and this shewed such a current as that indicated by the arrows.

10202. Tried the effect of the steel springs alone, i.e. without magnets or core, but with the helix in place. They produced a current due to their alternate approximation and recession.

10203. Pulled them back so as to make them act with short lengths and farther off from the helix, and then the effect was very much diminished.

10204. Employed two plates of copper for copper springs, and these, when out full length, gave no effect at the galvanometer. So these, being hardened by hammering, will do instead of the steel.

10205. Must strut the table well and also weight it well and fix all parts steadily.

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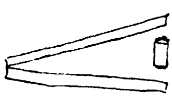
10206. Have strutted and blocked up the table on which the machine rests by horizontal timbers so as to take all shake or vibratory twist away, and also place[d] two 56 lb. weights of iron on it so as to make every thing steady. There seems now to be no shake even when working most vigorously.

10207. Connected the Galvanometer, commutator and machine and helix as it was yesterday (10189), tried the circuit and found it perfect. The machine is and has been for the last days HORIZONTAL. The copper springs (10204) are in use.

10208. Now working the machine with the helix but without core or magnets—there was *no* action at the Galvanometer. This I believe to be a true result and it shews that there is no generation of a current at the commutator or elsewhere.



10209. Now put the bar magnets into place. The helix is that containing 516 feet of fine wire, and neither it or the magnets move if all is right. Worked the machine hard, but there was no signs of any effect at the Galvanometer.

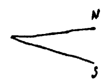
10210. Without one of the struts there was a little shake, and then a current was induced—but when all was immoveable except the carrying arms, etc. of the machine, there was no effect

produced. It is manifest that with Magnets and helix at rest, no motion of the other parts is able to produce a current.

10211. The magnets are on a stool independant of the machine table, and that stool is itself *strutted* and *weighted* and is very steady.

10212. The machine being in this unexceptionable condition, the *bismuth core* (10179, 10187) was placed between the carrying arms within the helix, so that the *core*, *helix* and *magnets* were combined for the effect. The circuit was found by trial to be perfect. The machine was then well worked–but no induced current was created. Then the Magnetic poles were reversed, but still there was no effect. I cannot in this way make *bismuth*, yet I think it ought to become so: if Weber's results are correct and my apparatus delicate enough.

10213. *Copper core*, *helix* and *magnets*. When the magnets were thus, there was no sign of action, but when reversed, there was the smallest possible trace of a current–the end of the needle going East when D was connected with A and C with B, see (10201). Must see that this is not accidental.



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10214. Have added brass springs to the copper springs. Have fixed the helix by a clamp. Have retouched the needles of the Galvanometer and made them so equal or nearly so, as to be more than thrice as long in vibrating as they were before, so that now the instrument is very delicate. Arranged all, i.e. coil, magnets, springs, etc. but *no core*–and found connexion good. On working the machine–obtained no deflexion of the needle, so that all so far good and free from sources of error.

10215. Placed the *bismuth* core (10212) in the machine and then worked it. The needle end went West when B and C were connected and also A and D. Reversed the Magnet poles and again obtained an effect even more than before, but in the *same* direction.

10215[1]. Thought this must be due to some source of error and by degrees traced it to the shifting of the machine on the table, which, altering the relative position of the helix on it and the magnets, caused the effect. Strutted the machine on the table

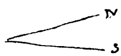
[1] 10215 is repeated in the MS. and 10216 omitted.

therefore, and now found it could be worked quickly without any change of place.

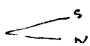
10217. Again worked the machine with the *bismuth core* and the magnets thus*. The needle end distinctly went *West*, when C and B were connected and also D and A. Then changed the magnetic poles to†, and now the needle went as clearly East in the same position of connexions—or the contrary to the former direction.

10218. Tied a small unmagnetic steel needle on to the bismuth core without removing any thing, and now on working the machine the Galvanometer needle went well West, the poles being thus; so iron makes a deflexion in the *contrary direction* to that caused by bismuth—and when bismuth alone is used, reversion of the magnetic poles reverses the direction of the induced current.

10219. This looks like a true action of bismuth according to Weber's results, acting as a *diamagnetic* core.



10220. Again *bismuth core alone*. Magnetic poles thus. End of Galvanometer needle went East. Changed the magnetic poles to thus—and now the end of the needle went west—connexions in both cases being thus $\begin{matrix} D\text{—}A \\ C\text{—}B \end{matrix}$; in which state I always purpose noting the *direction* of the deflexion.



10221. Removed the bismuth core and worked without any—could obtain *no effect* at the galvanometer. Good.

10222. When the magnets are in place, the mere once introducing a small needle into the core can affect the Galvanometer needle—but if I work the machine with the needle attached to the bismuth core, I can easily deflect the needle 90° and hold it there by the ferro magnetic action. This shews how well the machine does its part in throwing the alternating action into one continuous current.

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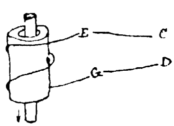
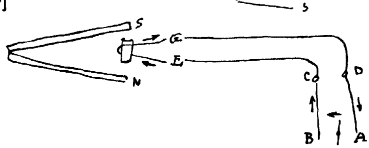
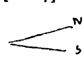


10223. As a record of the state of the connexions in the machine, I may state that the position of the commutator on it is such and it so changes that (as in the figures) when the core is moving downwards, the connexions are E—C and G—D; but that as it moves upwards, the connexions are E—D and G—C‡ (10201).

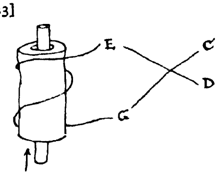
* [10217]



† [10217]



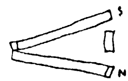
‡ [10223]



10224. *Bismuth core* in the machine–with helix–and both the brass and copper sets of springs. There was fair steady action at the galvanometer and the needle went *East* with D—A and C—B connexions. The magnets were there thus.

10225. On introducing once a piece of *iron wire*, that it might truly represent the *bismuth core*–the needle end went West, or the contrary to the bismuth direction.

10226. Changed the magnet poles thus–and then the use of the bismuth core sent the Galvr. needle end *West*: whereas a piece of iron introduced at the same times in the same direction sent it to the East.



10227. All this seems very consistent with Weber's results and a true diamagnetic action.

10228. Arranged the two bar magnets so that both the similar poles were together and on one side, thus–and the *bismuth* core being in the machine, the effect was very large at the Galvanometer, being 5° or more to the *West* by one contact at the Galvanometer commutator. Then placed the same poles at the other side or end of the helix and core. There was an effect also, but still the needle went *West*. Returned the magnets to their former position–still the needle went *West*.

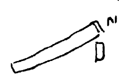
10229. Removed the bismuth core, leaving the magnets unaltered, and now without a core the needle end went feebly towards the *East*.

10230. So the effect is doubtless not so much due to a true bismuth diamagnetic action as to some slow or alternatg. shift of the relative positions of the helix and the poles, caused by working the machine.

10231. I now clamped the two conjoined magnetic poles to the frame of the machine, and as the helix is also clamped to this frame, I hoped that their relative position would not alter. Then worked the machine with *no* core in and obtained no result at galvanometer. Introduced the *bismuth* core, and then needle end went *West*. Placed and clamped the same pole at the other side and still needle end went *West*–a little.

10232. Clamped the two N poles thus–and with the bismuth core had no effect at all.

10233. Clamped the N poles on the contrary side, and now the needle went a little east.



10234. Now used single poles as before, clamping them down on

a level with the helix and core, the latter being *bismuth*; the needle went slowly and well to the *East* and in such a way as to make me think it a true diamagnetic action. Changed and clamped the poles thus*, and now the needle was as regularly to the *West*. Then removed both magnets and left the bismuth to act alone. There was no action at the galvanometer, and all this is very consistent.

10235. The *copper core* (10174, 10187) was put in the machine but without magnets–there was no effect at the needle. The magnets were clamped in their places thus–and the needle went a little to the *West*. Changed the magnets to contrary position and now the needle end went *East*.

10236. All this is like bismuth and unlike iron and looks very regular, but still fearing that it might be due to a vibration caused by the momentum of the moving masses, which might make the helix move one way and the other between the magnetic poles, by a very small quantity indeed, but one which would be gathered up by the isochronous action of the commutator, I put the copper core *outside* of the helix–thus–and still the needle end went to the East. This could not be a diamagnetic action, but probably such an one as I suppose. It is naturally greater when a core is in the machine because then the momentum of the moving parts is greater (10267).

10237. Blocked up the helix as well as I could between the Magnetic poles and now the effect was very much reduced, but still there was a little in the same direction as before.

10238. I now took the magnets away–placed the copper core in the machine with the helix round it, in the *horizontal* position, and worked the machine. There was no effect at the needle–really nothing. Turned the machine so that the helix and copper core could be *vertical* and now wrought the wheel, but there was no more effect than before. *Not a sign of any thing resulting from gravitating action.*

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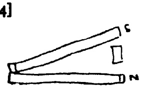
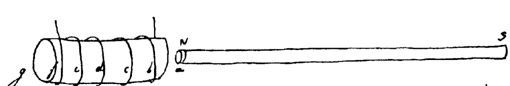
10239†. As to the action of my to and fro machine (10162, 10164) consider it a little. Whether the N pole be introduced from *a to b*, or *b to c*, or *c*—*d* or *d*—*e* or *e*—*f*, still the current induced in the helix should be the same; if the magnet were turned end for end as in the figure‡, and the same N pole again moved by *b*, *c*, *d*, *e*, *f*,

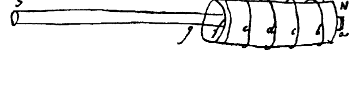
* [10234]



† [10239]



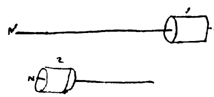
‡ [10239]



still the direction of the current in the helix should be as before. BECAUSE the wires of the helix are in all these cases cutting the magnetic curves of the same kind in the same direction. By experiment with the helix and a long small magnet (a long magnetic needle), this is found to be fact.

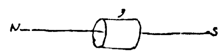
10240. So in my machine an iron core can act only by the superiority for the time of the end which is entering the helix, the magnetic line abutting on it being for the time the most powerful; or perhaps only by the parts which are entering after the commutator has changed.

10241*. A magnet passing from the first position to the second should produce *no final result*, for the N pole should do as much in one direction as the S pole in the other. And this by experiment is found to be the case; there is a momentary advance of the Galvanometer needle one way and then it instantly stops and is left almost still. Whether the motion is from position 1 to position 2 or from 2 to 1, the first motion of the Galvanometer needle is in the *same direction*, as it ought to be. Changing the needle end for end of course changes this direction.



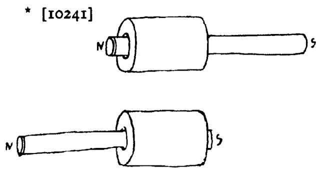
10242. If the magnet is moved from position 1 into position 2 and immediately back again into position 1, there is no final result of deflection.

10243. But if the magnet be moved from position 1 to position 3, there is a deflexion in one direction, and then if the magnet be carried on to position 2, there is a deflection the other way. So also from pos. 2 to pos. 3 gives a deflection the first way, and from pos. 3 to pos. 1 produces a deflection in the second way. So in going from position 1 to 2 and back again (10242), there are four alternate currents $\downarrow\uparrow\downarrow\uparrow$, and the sum of these is equal to 0.



10244. Hence my machine commutator is bad for magnetic and diamagnetic effects (though it may do for Gravitating experiments) and only a very small amount of the whole power (none theoretically) ought to take the form of one regular current. For the commutator divides the cycle of four periods into two, and though the divisions do not coincide with the beginning or middle of one direction of motion, still the two parts are equal to each other and each includes three currents the sum of which is equal to 0.

* [10241]



10245. For these experiments I want a commutator which can divide the to and fro into four equal parts, or else another in addition to the one I have, which shall divide the *to* into two and also the *fro* into two—and so gather up all the currents. Must get Newman to add the second to my present machine, after I have finished the Gravity experiments (10261).

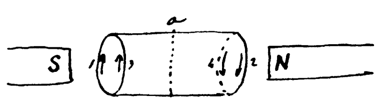
10246. But let us consider a revolving machine, after the fashion of Saxton's—only intended for diamagnetic bodies.

10247*. If the helix revolves on the axis *a*, as the wires 1 cut the magnetic lines of force, a current will tend to form in them—and as the wires 3 cut the same lines of force, an equal current will tend to form in them, opposed to the first and exactly equal to it. So if the helix passes by a half revolution from one equatorial position to another, the two sets of currents, of equal force in opposite directions, will perfectly neutralize each other. As wires 2 and 4 revolve, they will do the same thing. As the helix revolves, the currents that tend to form in wires 1 and 2 accord, for being on opposite sides of the axis of the helix, they at the same moment cut the lines of magnetic force in opposite directions; the same is true of wires 3 and 4.

10248. If the helix revolves slowly, the wires 1 and 2 coming first into the line of maximum force, will surpass in action wires 3 and 4, and so their current will be superior for a time: until they receed and wires 3 and 4 come up into the maximum force lines, when they in turn will have the maximum effect. Still, the sum of all the forces in one complete revolution will be 0. All these results were verified by experiment.

10249. But there are not four currents produced in one complete revolution, but only two, for as the helix revolves from one equatorial position to another equatorial position thrgh. 180°, there are indeed two currents ↑↓ the sum of which is equal to 0°; but as it revolves from one axial position thrgh. 180° to the next axial position, there is but one current, equal to the sum of two ↑↑. And this should be so, for if wires 3 and 4 as the helix begins to move make a current in one direction in the wire of the helix, so wires 1 and 2 at the end of the movement make a current in the *same* direction as far as regards the wire, though in the contrary direction as far as respects the ends of the helix;

* [10247]



for the ends of the helix are changed as regards the Mag. poles, but the ends of the wire are not changed as respects the Galvanometer. And experiment shewed this to be the case.

10250. So a complete revolution of the helix gives powers which are equal to 0, but they are thus if the position started from is the axial position, ↑↑↓↓—or if the position started from be the equatorial position, they are thus, ↓↑↑↓.

10251. If an iron core be in the helix, the effects are exactly and experimentally the same, and hence a commutator changing the direction at each axial position gives either with or without a core the maximum result.

10252. Hence there is no means of so dividing the rotation by a commutator as to render the effect of a mere helix nul without also at the same time nullifying the effect of any core placed in it: hence there is no chance of any useful diamagnetic machine to be obtained this way. The core and the Magnetic poles must be fixtures relative to each other.

10253. Now made some experiments in relation to *Gravity* with the machine vertical, the helix and core being also vertical. The *copper core* was in place and the *helix* fixed. There was *no effect—no result* due to Gravity.

10254. An *iron core* was introduced, the position being as just described (10253). The Galvanometer needle went fairly and well to the *East*. On reversing the position of the core the needle went equally to the West. So the iron is a feeble Magnet of itself, independant of the earth, and can evolve an induced current under this condition of the commutator and machine.

10255. Bismuth core (10212) put into the machine—all else being the same (10253). No effect.

10256. I now took the helix off from its support and by corks, etc. packed it on to the *copper core*, so that it and the core should move together and have no motion relative to each other—position as before (10253). Now there was a little motion of the Galvanometer needle, the pointer end going West when D joined A and C—B (10191, 10320).

10257. Thought it possible that this might be due to the condition of things which makes the helix, in moving to and fro, move in part of a circuit. The arm which carries the core and

now the helix is $22\frac{1}{4}$ inches long to middle of the core and the outside diameter of the helix is two inches: the radius of the outside part is 23 inches and of the inner part only 21 inches, and hence in its motion these parts travel with velocities which are in these proportions. Now as the machine is vertical and the line of the dip is not so, but is inclined about 69° or thereaway, the helix in its motion cuts these lines and so will generate a current due to the difference of 21 and 23: this is continually gathered up by the commutator and is probably the effect obtained at the Galvanometer needle.

10258. Supposing this the true account, then if the helix were placed in a horizontal position, the machine being vertical, so that as it moves it should move in part of a vertical circle, it would in its motion cut the lines of magnetic force of the earth in the most favourable manner, or directly across them. And having arranged things in this manner, the needle was now considerably affected, requiring no use of the Galvanometer commutator but going 6° or more *West* by a single contact of C—B and D—A.

10259. The copper core here ought to go for nothing. Replaced it by a wooden core and found that the effect was just the same in amount and direction (10320).

Now the motion of the helix and position of the connexions were as follows*:

10260. Now this is exactly the direction which the current should take, being induced by the magnetic lines of force of the earth.

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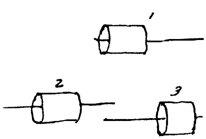
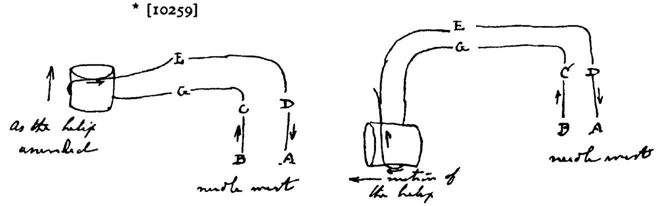
10261. The machine has now two commutators (10245) and I can change the direction of the connexion between the helix and the Galvanometer four times in a to and fro as required (10244). It was adjusted and it was ascertained that the feeble thermo current passed round the whole circuit in every position of the commutators.



10262. An iron wire was put in as core and moved thrgh. the four positions. Passing from 1 to 2 it gave one current ↑—from 2 to 3 it gave the reverse current ↓: from 3 to 2 it gave the first current ↑ and from 2 to 1 it gave the second current ↓—also the sum of these four current[s] gathered by simple wires to the galvanometer was ↑↓↑↓ = 0; but when gathered by means of the

* [10259]



two commutators, combined into one strong consistent current, $\uparrow\downarrow\uparrow\downarrow$ being made into $\uparrow\uparrow\uparrow\uparrow$.

10263. With the given position of the commutators, this current was such as to send the index end of Galvanometer needle East when the connexions at the Galvanometer were A—D and B—C, and this effect was due not to the iron wire as a magnet of itself, but as a magnet under the influence of the fixed bar poles (10234), for they were in place; and when the wire core was turned end for end–still the deflexion was in the same direction as before–so not due to the changed wire but to the fixed poles.

10264. A couple of sheets of foolscap paper were rolled up into a core the size of the copper core and employed: they caused the needle to deviate 3° or 4° and in the same direction as iron. Hence it acted as a magnet and I have always found foolscap magnetic. It gives an indication of the delicacy of the apparatus (10314).

10265. *Bismuth core in* (10187). No sensible action (10308).

10266. *Copper core in* (10187). Least possible trace of action–but the direction of the current produced was the same as for iron and foolscap paper (10264, 10309).

10267. I now put the copper core outside the helix as before (10236) and still had the same slight action. Removed the core and had none, and there was still the same feeble trace of action.

10268. I then observed that the outer ends of the magnets had moved, and all the appearances thus far are against any true diamagnetic results and seem to shew that the little action obtained is accidental and due to motion of the magnets and the helix relatively.

10269. I begin to despair of finding the diamagnetic action but will strengthen up the Magnets and try again.

10270. Tried to obtain the muscular galvanic effect spoken of by Magnus as produced at Berlin by   and witnessed by Humboldt and others. The large platina electrodes have had their upper parts varnished and well dried–then the lower parts or blades were well cleaned with a little emery, a cork and a solution of brine, and whilst wet put into a vessel of brine and connected and so left to equalize for 3 or 4 days. Two large clean earthen pans (footbaths), very clean, were filled with brine equal in purity

and strength, and the electrodes connected with my Galvanometer and also with the brine, so that the surface of the latter came over the varnished part; that the portion in contact with the brine might be invariable. The hands were then well washed and rubbed in another portion of the same brine and at last immersed one in each pan.

10271. The first immersion caused strong deflection of the needle, but when both hands were well in and kept still, the needle quickly came to zero. But then found many sources of current: thus rubbing the finger and thumb together at the bottom of the brine sent the needle off, and the connexion was such that when the rubbing was of the left hand the needle went to the right, and when of the right, the needle went to the left. I will call this going in the opposite direction. Agitation of the whole hand in the brine, either right or left, sent the needle in the opposite direction. Putting the hand deeper in on one side sent the needle to the opposite side—taking the hand partly out did not affect the needle sensibly, but after waiting a few moments and putting it in again, the needle was affected.

10272. Striving to keep the hands steady and without motion in the solution and then putting muscular tension on to the muscles of the upper arm (which from a boy I have been able to do with much readiness and force), I obtained a little motion of the needle to the opposite side, which I could make more evident by alternating the action of the arms—but it was very little, and that little seemed referable to the inevitable slight agitation of the hands in the brine, for it was in the same direction; and I found that after a prolonged rest in the brine, a very little motion of either hand would produce a small current.

10273. The longer the hands were in the brine and the more they were soaked, the less were the currents produced by motion, etc. as above. Also, AFTER much rubbing and agitation, the currents so produced were less. At these times the current supposed to be due to the exertion of muscular power was not sensible.

10274. I believe that there was no production of muscular or nervous current but simply those effect[s] due to mixtion of the solution *near*, *at* and *in* the skin of the immersed hands, which would differ not only in temperature but also importantly in their

saline condition. If this were so, then removing the brine from the basins and using common water there ought to invert all these currents, for my hands in their soaked or even in their natural state would be in the condition of saline solution to the ambient fluid, whereas before they were in the condition of water to it. So arranged things thus, and also washed my hands moderately in simple water, to render their outsides nearly as the water in the pans.

10275. There was needle motion at first immersion, but when the hands were quiet the needle quickly took its place at 0°. Then rubbed the finger and thumb together, and now the needle moved freely but in a direction the *opposite* to that it took before. This happened again and again—and also motion; a lifting out, etc. produced the deviations but all were reversed (10271). Now any current due to muscular action would not be changed under these conditions and so when all was quiet, I tried to obtain such an effect, but I could obtain nothing. I prolonged the soaking, until the strength of the deviations spoken of above were much deminished—but in no case could I obtain any evidence of current from the exertion of muscular energy.

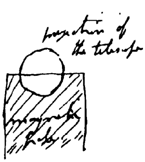
10276. I believe that my galvanometer is quite sensible enough. It is the sources of error that require attention. At present I do not believe in the effect.

10 OCTR. 1849.

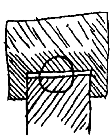
10277. *State of Gaseous diamagnetic bodies at the Magnetic poles* (10301). I received Plateau's letter[1], dated 25 Mar. 1849, only on the 29th Septr., and though I had made certain experiments formerly of the same kind (   ), resolved to repeat and extend them as well as I could. I first arranged to experiment in air. So I borrowed a fine telescope from Sir James South, having an object glass of 3 inches aperture and about 46 inches focus and furnished with a perfect micrometer of Spider's web lines. The large Electromagnet was used, excited by 20 pair of Grove's plates—and the object was a star of light formed by making a pin hole in a stretched sheet of brown paper and placing an Argand lamp behind it. The experiments were made in the night.

The position of things was thus*: so that the ray from the pin hole grazed the surfaces of the Electromagnet and then entered the telescope to pass to the eye. The distances are set down above[2].

10278. Having adjusted the telescope so that the micrometer threads and also the artificial star were perfectly distinct and occurred on[e] on the other, subject to no displacement by the mere motion of the eye, I then put the Electric current on to the magnet, but could not discover that the object (star or light) was in the least affected by it, either in place or character; whether on putting on or taking off the magnetic force.

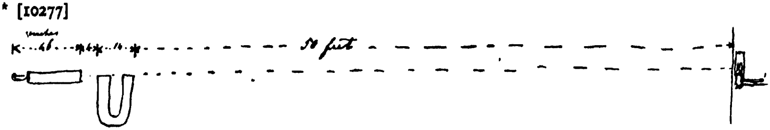


10279. But as many rays would pass from the star to the telescope at some distance over the surface of the magnetic pole, and these would make up much of the image in the eye, and might by their lustre overpower the part of the light passing close along the surface of the pole, I put up a screen of card on the pole, so as to allow that light to pass which was within $\frac{1}{8}$ of an inch or less from its surface. The brightness of the object was very much less than before, and by inflection it was lengthened upwards and downwards. Still, it was steady in position and character—the micrometer thread was well over it and any motion or change in it would have been very perceptible. But no such change occurred on the putting on or taking off of the magnetic force.

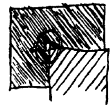


[1] See page 196 for the letter. [2] In the diagram.

* [10277]



10280. I then made the ray pass along the edge of the pole, as the place where the increase or diminution of density of the air would be greatest (if any), adjusting a screen there with a square hole about the $\frac{1}{8}$ of an inch in diameter; but still I could obtain no sensible effect.



10281. Considering that much light would pass at a sensible distance from the pole (as $\frac{1}{8}$ of an inch for instance), and finding that I could see the star object well through a small pin hole in a card, I dismissed the telescope for a time and used such a pierced card, fixing it to the side of the pole so that the ray coming to the eye should not be, at the most, more than the diameter of the hole ($\frac{1}{50}$ or $\frac{1}{100}$ of an inch) from the face or edge of the pole. But I could observe no trace of action—when the magnetism was put on or taken off. And yet the ray was in these cases conducted over the faces of both poles and also along the edge of one, and also along the straight edge of a piece of soft iron 7 inches long placed on one of the poles. Besides this, the hole was sometimes adjusted by shifting it a little, that the object should be almost or entirely lost to the eye, but the magnetism then had no power to bring it into more distinct vision.

10282. I do not believe that, in the case of air, there is any alteration of the density of the air against the poles of the magnet that can be rendered sensible by these means.

10283. I think also that the eye and the card with pin hole is a better means of observation than the telescope and wires (10291).

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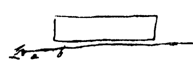
10284. Made a bar of iron red hot and then brought its side up towards the line joining the eye and a distant object (10281): the effect of the layer of hot or *expanded* air is to make the object approach apparently towards the approaching bar—and so it is put out or intercepted from the eye sooner by the hot bar than by a cold one. If a cold bar were so placed that it would almost but not quite exclude sight of the object, then being made hot whilst its position was unchanged, it would exclude the object, and as it cooled, the object would come into sight again. This is all right theoretically, for the ray proceeding from the object to the eye is curved in passing through the media of different density,



and the convexity is towards the hot iron, but the object, being seen in the direction of the part *a*, *b*, seems to be nearer to the approached iron than it really is–is in fact displaced towards it.

10285. If *diamagnetic gases* were made *rarer* near the surface of a magnetic pole, they should produce a corresponding deviation.

10286*. I then compared the observing power of the eye and the telescope (10283) by warming one of the long flat terminal pieces and letting the ray pass by its side, using the artificial star (10277) for object, and a card with a pin hole for the eye station (10281). The card was fixed on to the end of the terminal piece (iron and about 7 inches long), so that the pin hole was close to the side surface; and this, being 50 feet from the object, was adjusted so that the further end of the piece of iron all but extinguished the object. So now the ray must have grazed along the side of the piece of smooth iron. Then without the least change of the position of the iron or other things–the iron was warmed by a spirit lamp up to 100° or 120° F., but *no* disappearance or change of the object became visible. Then, the iron being hot, it was moved (the least possible degree) so as just to occult the object, and left to cool; but by cooling, the object did not become visible in the slightest degree. These experiments were repeated many times, using a large eye hole and putting it a little further off from the iron, so as to have all variations of position that might affect the ray and its course, but without any better result.

10287. Now as the temperature of 120° F. must cause such a change in the gravity of the air as would have been extravagantly indicated by a thermometer, so it would seem that in such a case as this, observation by refraction and the eye is not a good mode of looking for the effect, but a very bad one. It may be supposed that in a magnetic action the expansion or condensation of diamagnetic or magnetic gases may occur only very close to the surface; but there is no reason even to suppose this according to the ordinary law, for it is only at angles or points that the rapid change should occur. Over a plane surface where the lines of magnetic force do not sensibly vary in intensity at distances of $\frac{1}{10,000}$th or $\frac{1}{10}$th of an inch from it, neither should the density of a diamagnetic gas vary sensibly or in any degree more or less than the intensity of the diamagnetic or magnetic line of force. So

* [10286]

probably Plucker's plan by a thermometer form of arrangement is the best (714–90[1]).

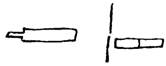
10288*. Now used the telescope (10277) in place of the simple eye to observe, leaving the screen with hole at the end of the iron to be heated or cooled. Or rather, a screen with a hole was set up so that the iron could be brought into place without disturbing the screen. The object end of the telescope was about 12 inches from the screen. The telescope was of no use without this screen, because if it were away, rays even an inch and a half from the face of the iron entered the telescope, and though a bright image of the object was obtained, it of course was immoveable. When the screen was there, with a hole about the $\frac{1}{16}$ or $\frac{1}{32}$ of an inch in diameter, a very faint image of the star object was obtained and it was blurred at the edges by the effect of inflexion; but still, it was an image formed by rays passing close by the iron when in its place.

10289. All being in order, the iron was made warm and brought slowly up to its place by the side of the ray. Within a certain distance it affected the image and then more and more as it moved on, until it put out the object by occultation. As the iron was withdrawn, the image reappeared and then was affected in the contrary direction. The effect was this: as the warm iron approached on one side of the ray, the object moved towards the other, or as if the iron had pushed it onwards; and as the iron was withdrawn, the object on reappearing seemed to follow its motion. But all this was in the dark, and as the telescope was an *inverting* telescope, so in it the iron, if seen, would be seen to approach on the opposite side, or on that side towards which the image appeared to move, and hence the results agree in direction with the former (10284).

10290. After numerous trials I found that, as a general result, the iron if cold did not cause motion of the object. The other piece of iron, if cold, did not move the object; if hot did. The end of the hot piece also moved the object. If the iron was only a little warm, it moved the object only a little. Two equal square blocks of lead were employed; when cold, they did not move the object, when warmed they did, though very irregular in

[1] ? pars. 10714–10790.

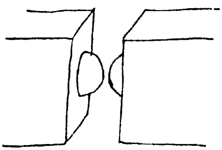
* [10288]



their surfaces, so as not to compare with the iron in that respect.

10291. So the effect is one of refraction by the expanded air—and such an effect can be distinguished by the use of the telescope when the unassisted eye can not distinguish it (10283).

10292. Now proceeded to observe with the telescope and other appliances just described, only passing the ray between the edges of two terminal pieces of this form* placed on the top of the Electro magnetic poles, so that it might pass from *a* to *b* just between the edges, the pieces being preserved from running together by a copper wire a little lower down, as at *c*. When the magnet was urged by 20 pr. of Grove's plates, the lines of mag. force must be very intense for these 2 inches and rapidly decrease in force outwards—and an ordinary diamagnetic would tend powerfully to *go out* there. But whether the magnetism was *on* or *off*, no influence or alteration could be discerned in the course of the ray or in the appearance of the object (10297).



10293. Replaced these edges by two raised hemispheres—and made the ray pass between them, but obtained no sensible effect.

10294. So I do not believe that in *air* any such effect is produced as expansion against the face of the magnetic pole, which can be rendered visible by refraction or divergence or inflexion of the ray.

10295. Still, I mean to try in atmospheres of oxygen, hydrogen and coal gas, because in *Air* the powers may be just neutralized of the oxygen and nitrogen (10298–301).

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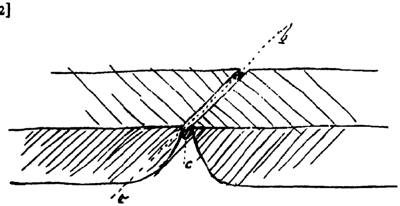
10296. Have arranged the two terminal pieces of old times (9231), facing them by two pieces of soft iron of the shape shewn over

* [10292]



leaf*, so as to permit of a ray of light passing close in the angle along the line of their edges from *a* to *b*; the distance of these two edges was about the $\frac{1}{16}$ of an inch and their length two inches, so that the ray was for that distance subject to their joint action. The French shade used before (9231) was put over them on a platform, and gas sent up into the space by a tube from an air-holder. When the lamp star object was in its place and also the screen with small hole in a line with the edges but a little above them (10292), and the telescope in place and use, I could see the object star, for though it was somewhat confused by the irregularities of the shade glass, still it was steady and easy to be observed.

The Magnet was urged by 20 pair of Grove's plates.

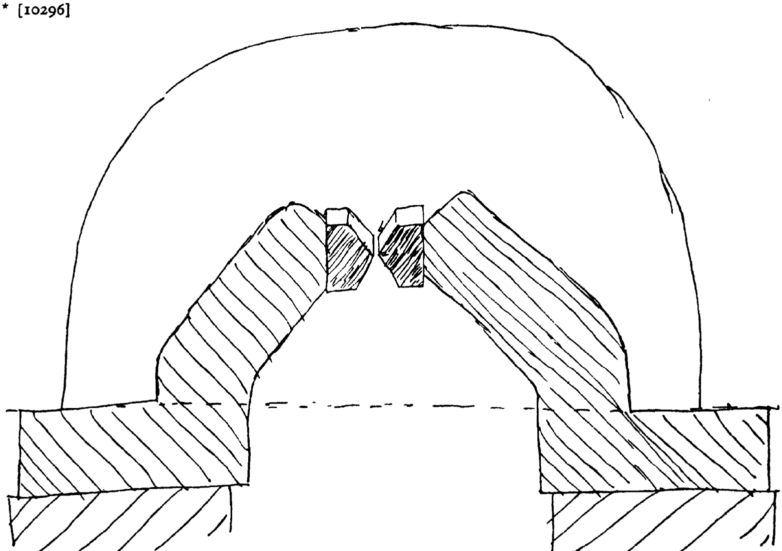
10297. With *common air* in the shade, no effect, whether the magnetism on or off—same as without the shade (10292).

10298. *Coal gas.* As with air, no effect, though shade had coal gas enough passed through it to fill it 3 or 4 times.

10299. *Hydrogen.* As with Air, no effect—same precautions as with coal gas.

10300. *Oxygen.* No effect—same precautions.

* [10296]



10301. So no results to be obtained this way: no sensible *condensation* or *expansion* of the air or gases in the *intense magnetic field* (10277).

[The original of the letter which follows is bound into Vol. v of the MS. at this point.]

See MS. Notes of Expts., 10 Octr. 1849, etc. (10277, etc. to 10301).

Gand, 25 Mars 1849

Mon cher Monsieur Faraday,

Permettez-moi de vous offrir un exemplaire du mémoire que je viens de publier. Ce travail constitue la suite de celui que j'ai eu l'honneur de vous envoyer il y a quelques années, et au sujet duquel vous avez bien voulu m'écrire une lettre flatteuse que je conserve comme un témoignage de vos bons sentiments pour moi. Dans ce premier mémoire, je n'ai guères eu recours qu'à l'expérience; aussi renferme-t-il plus d'une chose hasardée, et même de petites erreurs théoriques. Dans le mémoire actuel, au contraire, la théorie et l'expérience marchent de front, et se prêtent un mutuel appui. Vous y verrez se produire sur une grande échelle, des phénomènes de l'ordre de ceux auxquels on a donné l'épithète de *Capillaires* à cause de leur exiguité; vous y trouverez en même temps une suite de confirmations inattendues de l'admirable théorie sur laquelle repose l'explication des phénomènes capillaires; enfin, vous arriverez à une application qui consiste dans la théorie complète d'un phénomène dont l'étude expérimentale a formé la matière de l'un des plus beaux mémoires de Savart.

J'ai reçu l'exemplaire que vous avez bien voulu m'envoyer de votre mémoire sur les nouveaux phénomènes dont vous avez enrichi la science; je vous en remercie, et je saisis cette occasion un peu tardive de vous exprimer toute mon admiration pour ces brillantes découvertes. On pouvait penser que c'était assez pour votre gloire d'avoir ajouté à vos travaux antérieurs la découverte de l'induction électro-dynamique avec toutes ses conséquences si extraordinaires; eh bien non! voilà que vous constatez d'une manière inespérée l'influence des courants électriques sur la lumière, puis l'universalité de l'action du magnétisme. En vérité, créer une nouvelle branche de la physique, ce n'est qu'un jeu pour vous.

A propos de magnétisme, causons un peu, si vous le voulez bien. Vos belles expériences sur les gaz vous ont conduit à établir l'état magnétique ou dia-magnétique d'un gaz donné, par rapport à un autre gaz donné; mais, ainsi que vous l'avez fait remarquer, elles ne permettent pas de constater si tel gaz est, par lui-même, magnétique on dia-magnétique. Or, il m'a semblé qu'il serait possible d'arriver à cette connaissance absolue, et cela au moyen d'un procédé que je vais avoir l'honneur de vous soumettre.

Supposons l'électro-aimant renfermé dans une cage transparente remplie

d'un gaz donné. Si ce gaz est magnétique, ses molécules seront attirées par les pôles de l'électro-aimant, et elles seront, au contraire, repoussées si le gaz est dia-magnétique. Or, dans le premier cas, l'attraction des pôles aura nécessairement pour effet d'augmenter la densité du gaz autour de ces mêmes pôles, et, dans le second cas, la repulsion devra, au contraire, diminuer cette densité. Si donc le gaz est magnétique, la densité de la couche qui environne les pôles ira en croissant rapidement depuis une petite distance de la surface du métal jusqu'à cette même surface, et, si le gaz est dia-magnétique, ce sera un décroissement rapide de densité qui aura lieu. Par conséquent, lorsque un rayon lumineux traversera très obliquement la couche dont il s'agit, il sera quelque peu dévié dans un sens ou dans l'autre, suivant que la couche sera condensée ou dilatée.

Cela étant, et l'électro-aimant étant supposé vertical, placez verticalement derrière lui, à la distance d'une dixaine de pieds, par exemple, une feuille de papier blanc sur laquelle vous aurez marqué un point noir, et faites en sorte que ce point soit à la hauteur des pôles; puis, avant de faire agir le courant, placez-vous du côté opposé, à une distance au moins aussi grande de l'électro-aimant, et de manière que la droite qui va du point noir à votre œil rase la surface supérieure de l'un des pôles; enfin, faites agir le courant. Alors, si le gaz est magnétique, le point noir devra paraître s'élever un peu au-dessus de la surface du pôle, et, si le gaz est dia-magnétique, le point devra disparaître derrière ce même pôle.

Il est inutile de vous faire remarquer que, dans cette expérience, l'œil devra être bien immobile, et que, par conséquent, il faudra regarder à travers un petit trou percé dans une plaque portée par un support fixe. Il me semble, en outre, que les pôles ne devront pas être munis des armatures coniques dont vous vous êtes servi pour vos expériences: car les pointes de ces armatures étant tres voisines, le magnétisme de chacun des pôles doit être en partie dissimulé par celui de l'autre; je crois que les pôles devraient être terminés par des surfaces horizontales très légèrement convexes. Le plus ou moins d'effet dépendra surtout de la force de l'électro-aimant; mais je pense qu'on pourrait augmenter ce même effet, en plaçant les deux pôles suivant la ligne qui va du point noir à l'œil: car alors le rayon, après avoir traversé la couche qui environne l'un des pôles, traverserait ensuite celle qui environne l'autre, et sa déviation serait doublée. Peut-être aussi serait-il bon de remplacer le point noir par une ligne noire horizontale et suffisamment longue: cette ligne devrait paraître brisée. Si l'action était trop faible, vous pourriez regarder à travers une lunette munie d'un fil horizontal. Enfin, il est possible que l'action de la pile échauffe notablement les fils de l'électro-aimant, d'où résulterait un courant d'air ascendant et dilaté, qui pourrait devenir une cause de déviation du rayon lumineux; dans ce cas, il faudrait garantir les pôles de ce courant d'air chaud, au moyen d'écrans convenables.

Je vous expose ces idées telles qu'elles me sont venues à l'esprit, et vous en ferez l'usage qu'il vous plaira; seulement, si vous les mettez en pratique, j'attends de votre conte que vous me fassiez part des résultats positifs ou négatifs auxquels vous serez arrivé.

Puis-je espérer que vous aurez l'obligeance de faire remettre les exemplaires ci-joints à Sir J. Herschel et à Messieurs Wheatstone et Grove dont j'ignore les adresses, ainsi qu'à Société Royale?

Tout à vous

J. Plateau

professeur à l'Université, place du Casino 18.

P.S. Je m'aperçois qu'au commencement de ma lettre, en parlant de mon premier mémoire sur les masses liquides, je semble faire le procès aux méthodes expérimentales. Telle n'a pas été mon intention; j'ai trop souvent moi-même employé ces méthodes pour ne pas en reconnaître toute l'importance; j'ai voulu dire uniquement, que comme le sujet de ce premier mémoire pouvait être abordé à la fois par la théorie et par l'expérience, j'ai eu tort, dans ce cas, de m'en tenir à l'expérience seule.

10302*. *Falling bodies* (10082, 10018). I havė arranged two spring loops of copper wire, one above the other, with an interval of 3½ inches between them. Have a bright copper rod, ¾ of an inch in diameter and 24 inches long, which being introduced between the loops, is pressed against by them and in contact with them during the fall of the rod, until it is out of their contact and reach. The loops are connected with my Galvanometer and the contact is perfect, for a piece of iron wire applied as a thermo test current shews it so. Then the copper rod was allowed *to fall*, but no effect like an electric current was produced. None was expected, but I wished to be quite clear as to the natural result and condition of the parts.

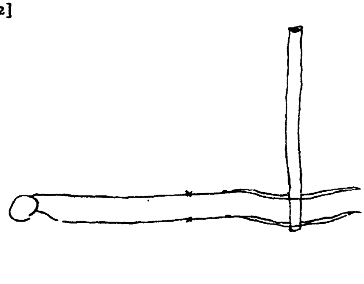
10303. In place of the two loops—a helix was substituted containing 516 feet of wire (10189). The rod in falling produced no effect (   ).

10304. By a particular arrangement the rod was rapidly rotated before it was allowed to fall, but whether rotating in the one direction or the other, no effect was produced.

10305. The copper rod was connected by a copper wire with one end of the Galvanometer wire, and the other Galvanometer wire with an excellent discharging train (the water pipes): the rod, whether falling with on[e] end or the other downwards, or whilst in a horizontal position, gave no current—none was to be expected.

10306. Whilst the Galvanometer was thus arranged, many curious sources of error made themselves manifest. Anderson, on touching the copper rod, caused strong deflection to the west; I caused none at all, and other curious results apprd., all of which were traced to this—that when the copper rod touched the floor of the Laboratory, there was a current thrgh. the Galvanometer due to the difference between it and the water pipes, etc. Touching the lead on the floor, or the stone, or wearing gutta percha soles, etc. made strong variations and would produce good lecture room experiments.

* [10302]



10307. Worked upon Diamagnetic action at the Machine (10162–4, 10239, etc.) having both commutators on as before (10261). The bar magnets have been much strengthened in force and are griped and held in their places at all the ends (10234). The machine is of course horizontal now—the connexions to the galvanometer good and all in order.

10308. *Bismuth* core in (10187, 10212, 55, 65). Can procure no signs of any action. A little piece of iron wire brought up to the helix produced an effect and when introduced into the helix produced good effect. So connexions are all right and by the non action there is proof that the magnetic poles and the helix are fixed relatively to each other—but then *bismuth* has no action.

10309. *Copper core* (10187, 266). A little effect—the needle end went West when C was connected with B, and D with A. But on putting in an iron wire core and moving it in the same direction with the same position of commutators and connexions, the motion, though far stronger than with the copper core, was in the same direction. So the copper acts magnetically and not diamagnetically—is probably somewhat impure from iron.

10310. *Platina—2 wires*—no sensible action.

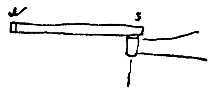
10311. *Flint Glass rod* as core—no sensible action.

10312. *Sulphur* core—no sensible action.

10313. Sol. (saturated) proto sulphate iron contained in a glass tube. *No effect*—very singular. Again and again no effect, yet the connexions were good throughout. Is this due to the fluid state? Or is this form of apparatus not good to make manifest the effect? Must consider that the Magnetic field is unchanged in intensity the whole time and the core always in it and nearly equally.

10314. *Foolscap paper core.* Nothing (10264). So put a single length of steel harpsichord wire in the middle of this core, but still no sensible effect. With a thicker wire $\frac{1}{20}$ of an inch in thickness had a good effect—and that in moving through all the four parts (10262) of a cycle of motion. Again used the associated foolscap paper and fine steel wire and now obtained a little effect and in the right direction—but nothing like so much as on former occasions or as I expected (10264, etc.). I think that the former results must be due to vibration, shakings or motion of the helix or magnets which are now prevented.

10315. Dismissed the machine. Placed the two similar poles of the bar magnets together, put the helix close up to them and then made connexion with the galvanometer. Then introduced the core of paper and wire (10314), making it pass up to the magnet, and then had deflection of the galvanometer needle *one way* as it entered and the *other way* as it was withdrawn.



10316. Could obtain no effect from the copper (10309), or the bismuth (10308) by this means. Still, it is manifest that this process is better than the use of the machine.

10317. In this respect see the expts. of (10330).

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10318. Arranged the Machine (10253) with one Commutator in a vertical plane, so that the cores might travel up and down vertically or nearly so within the helix, the latter being fixed—for results relative to Gravity. The commutator divided an up and down motion into two halves, reversing the direction when the up and down motion[s] were about $\frac{3}{4}$th over, so that the stoppage of the down motion and the coming on of the up motion made one period, and the stopping of the up motion and coming on of the down motion made the other (10165). The connexions were all Good.

10319. Now cores of bismuth and copper as heavy conducting bodies, and of Glass, sulphur, shell lac, gutta percha as insulating bodies, were employed, but no effect was produced at the Galvanometer. So Gravity does not come into relation with Electricity this way (10018, 61).

10320. Repeated the results of the helix on the Machine vibrating without a core and producing a current by cutting the curves of the earth's magnetic force (10256, etc.). Fixed the helix by corks as before, and having the machine and the helix horizontal and therefore the motion in a horizontal plane, no current was produced. When the machine was vertical and the helix also, and therefore the motion vertical, there was no sensible effect—the least possible trace. When the machine was vertical and the helix and motion horizontal, then as before a current was produced. When the helix was above the center of motion the current was one way; when below the center of motion it was the other way.

So all was consistent with theory and the known laws, and the effects are good illustrations of the currents produced by cutting the magnetic curves of the earth.

10321. The machine being as just described, if it were gradually depressed to the horizontal position, the current evolved became less and less and at last diminished to nothing.

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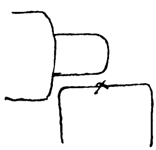
10321[1]*. Employed our large cylinder Electromagnet, urged by two pair of Grove's plates well charged, as a magnet. Had a long wire covered–reaching from my electrometer 33 feet off to the magnet and back again, in which any currents that might be induced were to be observed; the middle part of this wire or that at the magnet, which I will call *x*, was taken over the edge of a board about 3 inches wide and then by the sides of the board and so away to the Galvanometer; the board served as a handle or a foot and the part *x* could be placed and fixed in any position relative to the magnet.

10322. Another wire, arranged in a similar way on a board, was connected when necessary with a battery of five pair of Grove's plates so as to supply a wire carrying a current when needle[2]; the part corresponding to *x* I will call *y*. So *x* is the wire in which currents or effects were to be sought for and *y* is an electric current which, with the magnet, were to act on it.

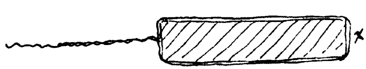


10323. Placed the wire *x* parallel to the magnet axis but by the side of the pole: then made the magnet active: that did not induce any current in *x*, for it and its prolongations are in a plane passing through the magnetic axis. Hence the opening magnetic curves would not cross the wire but form and move parallel to its plane, and this is a position of indifference. By raising or lowering *x*, it then cut the magnetic curves, and a weak current was induced. The effect was very small though visible–the magnetic pole is not strong enough. Considering that *x* is only 3 inches and that in the connecting wire and Galvanometer there are perhaps 600 or 700 feet of wire, more effect could not be expected.

10324. Then the current in *y* (10322) was approached to and

[1] 10321 is repeated in the MS. [2] ? needed.

* [10321]



removed from x: but there was no sensible effect either when the magnet was active or not. As there was no effect without the magnet, probably it could not be expected with it.

10325*. In place of wires x and y, used two flat copper wire helices containing each probably 32 feet of wire. Helix x was placed at the summit of the Magnetic pole, so that the magnetic axis was in its plane, and this being an indifferent position, the Galvanometer was not affected by making the magnet active or inactive. Helix y was then connected with the 5 pr. of plates (10322). This affected x by approximation and recession, as it ought to do, *but* when the magnet was made active there was *no difference*. No signs of a *permanent action* when the magnet was on.

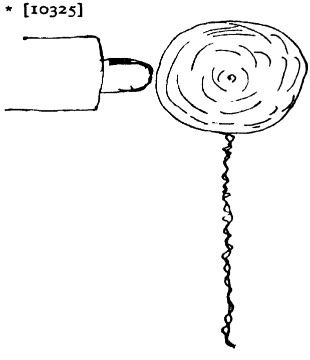
10326. Now put helix x with its plane tangential to the end of the pole and in contact with it—the pole and the helix were concentric. No permanent action. Then brought up helix y with its current. It acted on x exactly the same whether the magnet were active or not, and there was no permanent action. Nothing new.

10327. Dismissed helix y, and gave the whole power of the battery of five pair of plates to the Electromagnet. Retained helix x as just described, i.e. a tangential plane at the end of the magnetic pole. Then a piece of iron approached end on towards the magnetic pole, by concentrating the Magnetic curves, produced abundant electric current in x. But a good thick piece or bundle of bismuth bars did nothing of the kind. *No diamagnetic action.*

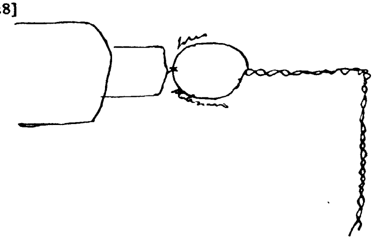
10328†. Made a thermo electric combination of Iron and platinum (8427[1]) and replaced the helix at x by it, so that the loop should be in an indifferent position (10323). Then put on and off the Magnetic power. There was no action at the Galvanometer either

[1] ? par. 8426.

* [10325]



† [10328]



at the moment of charge or permanently. This negative result agrees with the former old experiments.

10329. Have a good thermo electric battery of 40 alternations. Placed this so that one set of terminations were close to the Magnetic pole and the other $1\frac{1}{2}$ inches away. *No action* either on making or breaking or continuing the battery contact with the magnet. Great care is required here to place the thermo electric battery and also its connexions in an indifferent position *first*.

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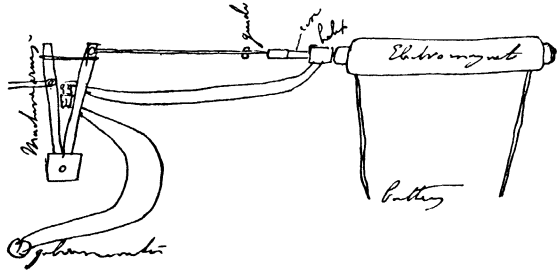
10330*. Have had the Machine arranged according to the principles indicated (10315, 6). A rod attached to the arm of the lever carries the bismuth or other core at its extremity—by the motion of the arm the core enters into and withdraws from the helix (10189), and the latter is close up to the end of the cylinder electro magnet (10321). The core in advancing passes almost into contact with the end of the electro-magnet and then withdraws from it, receeding about 2 inches. The magnet and the helix are, necessarily, fixtures in respect of each other; the core is the moving part. The magnet was urged by 3 pr. Grove's plates and was far superior in power to the two bar magnets, probably 10 or 15 fold as strong. The commutator divided the cycle as before described at (10318). The connexions were all good and a thermo current easily passed through them. The Galvanometer needle was displaced only 2° when the magnet was excited, being about 12 feet from it. For the purpose of avoiding the communication of tremor from the machine to the helix or magnet, the former was on the Lecture table in the lecture room and the two latter on a separate table—a guide which supported the rod was the only thing by which motion could be communicated to the helix table besides the floor.

* [10330]



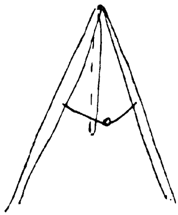
10331. *Bismuth core.* When the magnet was not excited, there was no effect produced at the Galvanometer by working the machine. The core probably performed 5 complete movements into and out of the helix per second, i.e. approached the magnet five times and was withdrawn five times. Again, if the magnet was excited and the machine *not* worked, there was no action at the galvanometer save the 2° of displacement (10330).

10332. When the magnet was excited and the machine worked, there was abundant action at the needle, but this was soon referred to causes of error: it was sometimes one way and sometimes the other—and it diminished very greatly when the wire guide was unclamped from the table, and from all the appearances I concluded that it was the effect of a tremor or other motion varying the position of the helix and magnet isochronously with the inversions of the commutator.

10333. When the bismuth core was away and the machine worked with the excited magnet, there was very little effect at the Galvanometer though the helix and magnet were in place, but the carrying rod did not then rub with the same force upon the guide (10330). When a spring of wood was made to press upon the rod, still there was no effect, but also there was not the same shake as when the heavy core was carried by the rod, for its stopping suddenly made a considerable tremor.

10334. Tied the copper core on cross ways to the end of the carrying wire and cap, so that there might be the same mass moved but yet the core be out of the helix, and *now* there was much deflexion at the Galvanometer. So believe that all the action was from Sources of error.

10335. Sometimes at the beginning of working, when all had been just and well arranged, there was no effect at the Galvanometer though the bismuth core passed to and fro in the helix under the influence of the magnet. *I think at these times there would have been signs if the bismuth had been able to induce a contrary current to iron.* In a few moments vibration of the whole table came on and then the abundant but irregular action due as I think to sources of error.



10336. Further precautions. Instead of letting the core rod rest on a guide on the table, I have erected a triangle on the floor over the table and by a fine copper wire made fast to two of the legs and passing once round and under the core, I have slung it so that it can move only parallel to itself and yet its weight is not on the table sustaining the helix and magnet. Have also fixed the axis screw of the machine. Have adjusted the commutator so as to change at the beginning and also at the end of one direct *to* or *fro* motion. The machine works very steadily now, and the connexions with the Galvanometer are perfect. The helix and Magnet are bound together and 2 pr. of plates used for the magnet.

10337. *Bismuth core* in its place. I think it does not touch the helix or its table any where. When the machine was worked without the magnet there was no effect at Galvr. When the Magnet was excited without working the machine, the needle was deflected as yesterday about 2°. Still, on using both Magnet and machine there were some irregularities of action. So strutted the magnet table better and now all looked well. When the machine worked, there was a little regular motion of the needle, slow and steady, looking like real bismuth action. The end of the Galvanometer needle went West when the outer terminations were connected according to the standard condition ( ) at the Galvanometer.

10338. A piece of iron wire of equal length with the bismuth core, being attached to it so as to act as an iron core, did do so powerfully, and the needle end went Eastwards. *This is therefore the standard of direction for Magnetic action* with these connexions.

10339. *Bismuth core again*, iron being away. The action now irregular and that two and three times in succession, needle end going sometimes west and sometimes east with same galvanometer contacts. This must be due to some irregular motion at the helix. Then again a very regular case came on of needle end to the *West* (10397, 416).

10340. *Copper core in place.* Needle end went well regularly and strongly to the West. Again, with a slower motion, same effect. This happened a third and a fourth time. Is this a peculiar effect of copper or is it some error (10382)?

10341. *Gutta Percha core.* Nothing. No direct effect and no error. So the shake of the machine with a light core in produces no error. So that error in any case cannot well be through motion communicated through the floor.
10342. *Sulphur in glass tube.* The least degree of effect. The needle end to the west as with copper.
10343. Suspect still a knock or a slight rub of the heavy cores against the sides of the helix; must have the copper and bismuth made smaller and quite clear of the helix, for Monday.
10344. *Foolscap paper core*—no effect.
10345. *Platina wire core*—Do.
10346. Copper core again—well fitted and centralized, gave same effect as before.

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10347. Have adjusted the commutator—and made machine more steady and connexions are good with Galvanometer. Helix is steady to the magnet and the cores of copper and bismuth have been turned smaller so as to leave no fear of their striking or rubbing inside of the helix as they pass in and out.
10348. *Copper core* in and well centered. When the machine was worked, the near needle end went at once and well to the *West.* This happened again and again. I could feel no vibration or knock on the helix by the finger and I find that is a very sensible test of rubbing or a blow there. Even when motion of the machine was slow, the needle end went West with the standard condition of contact at the Galvanometer. The fact very clear—is it due to iron or what? (10371, 10382).
10349. *Iron.* Placed a wire of iron alongside and with the copper core and again worked the machine. The needle end went *Eastward*, very powerfully. This therefore is the action and effect of *iron.* I believe the other is the true action of copper, and as it is contrary to the iron action, it cannot be due to iron or ordinary magnetic impurity in the copper. Must either upset or establish this point and find its relation to supposed polarity of bismuth in Weber's experiments.
10350. *Bismuth core*—attached and centered unexceptionably, but there is no effect at the Galvanometer. This happened again and

again. So the copper effect is not the bismuth effect and probably not a diamagnetic effect; but what is it? (10339, 10397).

10351. Is not the effect due to currents induced in the approaching and receeding metal, which would be of course concentric with the prolonged axis of the Magnet (the electro cylinder urged by 2 pr. of Grove's plates (10330)), and is not the reason of the activity of Copper and the inactivity of bismuth to be found in the difference of conducting power, just as that produces differences in revulsive effects (10408), and are not the revulsive effects and these due to one and the same mode of action and so the revulsion phenomena proved and confirmed?

10352. Iron manifestly acts by its magnetic condition—being a bad conductor it would allow of a very feeble development only of these currents—but that would be almost infinitely overpowered by its results of true or especial magnetic force. But then, if bismuth were rendered polar reversely to iron, it ought in conjunction with these induced currents to shew some effect surely; and that it does not seems to indicate that it is not reversely polar to iron when near a magnet or in the magnetic field.

10353. In the non production of these phenomena by badly conducting metal we see how *time* comes in as an element related to and connected with conducting power. For the currents could probably be induced in bismuth if time were allowed or could be allowed in proportion to its bad conduction power.

10354. Bismuth is crystallized and Magnecrystallic, but the core is a mass of crystals not symmetrically arranged but in all directions. It would be a great point to ascertain the effect of a core of bismuth built up of crystals, all the axes of which should be parallel to the axis of the core.

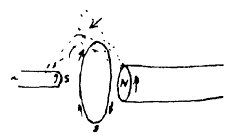
10355. If my view right, then a core of copper *wire* parallel to the axis should give nothing or next to nothing—a core of copper discs should give the effect—and a core consisting of a solid copper wire helix should give the effect without the external helix, i.e. by itself. Also such a core should give no effect in the helix if the ends not connected and should give the effect if the ends are connected.

10356. So also (perhaps?) if tin and lead cores, being solid, do not give the effect, cores built up of disc[s] of tin and lead ought

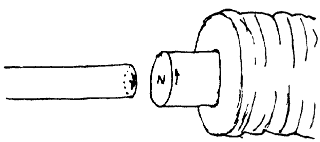
to do it, just as they give powerfully the revulsion effect—which by such means can be made sensible when it cannot be found in massive pieces.

10357. So the copper in these experiments puts on the diamagnetic character of polarity apparently instead of the bismuth, but it is not in reality a polarity, or at least not a polarity antithetical to that of iron or magnetic bodies, and though it simulates the action of a magnet (in the contrary direction), it is by another mode of action. For as the copper cylindrical core *approaches* towards the end of the magnetic iron core of the Electromagnet, the electric currents induced in it will be the reverse of those which correspd. to the typical current in the magnet as in the figure*; whereas as the copper core receeds from the pole, they will be in the *same* direction relative to the joint axis of the two cylinders, the core and the magnet. With Iron, the currents which act are always in the same direction, namely as those in the magnet—but they strength[en] on approach and weaken on recession. With a diamagnetic body truly polar antithetically as Iron is, they would be always the contrary to those in the magnet—also strengthening on approach and weakening on recession. With the copper there are both currents, so that it is as the supposed polarity of bismuth as it approaches and as the polarity of iron as it receeds.

10358. Now whether a piece of iron, as an iron wire core *a*, when approached towards the N pole of a magnet, be supposed to act directly by its own inducing force (as an S pole) on the helix spirals *s*, or whether it be conceived of as causing the magnetic lines of force emanating from N to draw in or collapse and so to travel across the spiral, the induced current will in both cases be in the same direction and as represented in the figure. If on the other hand *a* were a core of bismuth that could assume reverse polarity and so have an N pole raised up opposite the N Electro magnetic pole, still that pole, acting either directly on the spiral in its motion or acting by diverging, opening out and diluting the lines of force from N passing across the magnetic field, would in either or both cases produce the same effect, i.e. a current in the contrary direction to that figured above. But a core of copper cannot be conceived of as acting in such a way, for though as it approaches the currents set up in it are contrary to those in the

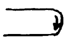


* [10357]



magnet, and therefore the same as those supposed in the bismuth, *they do not continue*; and the moment the advance ceases they cease too, and the copper is neither as iron or bismuth therefore; the magnetic curves in the magnetic field are not either converged or diverged as they would be with iron or bismuth core, but exactly (as is supposed) as they were before the copper was there or advanced. On the recession of the copper the reverse happens to exactly the same amount.

10359*. So the advance and recession of the iron first concentrates and then diverges the lines of force as respects the helix, which gives rise to currents in two directions, and these by the commutator are gathered up into one consistent current. The advance and recession of the bismuth should also diverge and then concentrate the lines of force and so give two currents in contrary direction to the former, which being gathered up by the commutator, should make a current the reverse of the former. What should the copper do? If it act by the currents set up in it, then as it approaches the magnetic pole, the currents formed in it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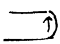
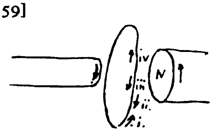
should be of the contrary direction to the magnet currents and should induce a contrary current to themselves in the helix, i: when it ceases to move, these currents in the copper will cease and their cessation will induce a current in their own direction in the helix, ii: as the copper receeds from the magnet, a new current will be set up in it, parallel to and in the same direction as that


iii

in the magnet, and this will induce in the helix wire a current, iii, in the contrary direction to itself: and when it stops again, the current in it will fall and cease and in falling will induce a current, iv, in its own direction in the helix wire. Thus there will be four currents induced in the helix wire in the cycle of motion $\uparrow\downarrow\downarrow\uparrow$, but the second and third of these will be in the same direction and coalesce into one, and the fourth and first will also be in the same direction and therefore coalesce into another; and these two in contrary direction will be gathered up by the commutator and converted into one regular current.

10360. Now the commutator is so arranged that the galvanometer connexions are inverted just before the motion *to* or *from* ceases (10437, 10463). Hence as the copper approaches, the current in the helix is carried off: just before the copper stops,

* [10359]



the commutator changes and the current due to stopping, ii, is then gathered up; then the copper is withdrawn and produces current iii in direction as ii, and the commutator still takes it up; but just as that current is about to cease and before the new one produced by the stopping of the copper, iv, exist[s], the commutator changes, and then gathering it up, joins it on to the one produced by the approximation of the copper in the next cycle of motion (10410).

10361. So the results are the *same* as if the copper were a magnet antithetical in its character to one of iron; but it is very manifest that it is not such a magnet, but acts on different principles. It can probably only act thus when a mass: a *mass made polar* for the time *in contrary directions*, whereas a magnet or a diamagnet should consist of particles or molecules made polar for the time and always in the same direction.

10362. No doubt a larger law of action would bring both or all three cases under one expression, but still that would not as yet shew that bismuth is a diamagnet—even though it might produce the phenomena of copper. It has as a diamagnet to produce the *antiphenomena* of iron.

10363. I must look at Weber's results to see how they build in with these considerations and what the results are.

10364. I made several experiments now with weak magnetic bodies, looking at the results with a very different mind to what I have had before.

10365. i. *Saturated solution of Sulphate of Iron* (*proto*) in a thin glass tube. Well arranged, but it produced no sensible action at the Galvanometer. The motion of the machine was not the quickest but pretty good.

10366. i. *Green bottle Glass*—a thick dark green tube—indifferent in action—if any result, it was in the least possible degree as Iron (10400).

10367. i. *Crystals of Proto Sul. Iron* in the tube. A doubtful trace of action—if any, is as iron but the action is very small or none (10401, 10435).

10368. i. *Iron filings* in the tube. Exceedingly powerful, as iron.

10369. i. *Iron scale oxide.* Do.

10370. i. *Red oxide. Colcothar.* Very feeble indeed—the least possible trace of action—as iron.

10371. i. *Copper core* again (10348). As before—contrary to iron and strongly; the needle end could be sent 30° West, whereas the solutions or crystals of iron and the Red oxide hardly sensible.

10372. Hence, either this machine gives a bad method of shewing magnetic polarity as compared to the mere act of attraction of a magnetic needle, or else *time is required.* Must use the crystals of sul. iron—slowly—to see what time does.

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10373. Whatever the final relation (10362), there is this difference probably between the inductive action in copper and magnetic including diamagnetic action (if it be as Weber thinks); that section across the core, as into discs, does not retard but may even help (as in revulsive phenomena) this action, whereas it interferes with magnetic action because of its polarity seriously. Also, that division parallel to the axis of the core, as when it consists of small wires, will interfere much with the first but not with the latter. This may depend in part upon the point that what is Massive in the one case is molecular in the other, namely the currents real or imaginary, and this may be connected with the point that one is polar and keeps its state, whilst the other is not, after the first moment, but falls to 0°.

10374. Is Magnetic action *across space*, through air, water, a vacuum, etc., but *between contiguous particles* in iron, nickel, etc.? And if so, can a given space, as that occupied by a piece of protoxide of iron, be traversed by the power partly in one way and partly in the other? And if so, what would be the condition of iron space as the iron is heated in the Mag. field from cold to white heat?

10375. What a strange thing an electric current in an electrolyte would then be and indeed is—acting across *space* laterally but from particle to particle only axially.

10376. Consider the motion of a magnetic pole and wire carrying an electric current round each other. The motion of the pole round the wire is a motion of the pole *along* lines of magnetic force. Its lines of magnetic force *cross* the wire lines of force.

10377. The lines of force of M. poles are flexible and change in their direction as another pole is brought near, so that there are obstacles in the way of working with two magnetic poles. But the lines of force in a wire carrying a current are probably pretty permanent, depending on the current. So consider state of and work with two currents in wires.

10378. Ought not two wires or currents at right angles tend to move in the direction of *their length*? Or if one fixed, ought not the other to move? If so, ought not such wires, one having a current and the other not, affect each other? And may not this be the relation which I have tried so long to discover (   ) in two such wires?

Consider and work out all this.

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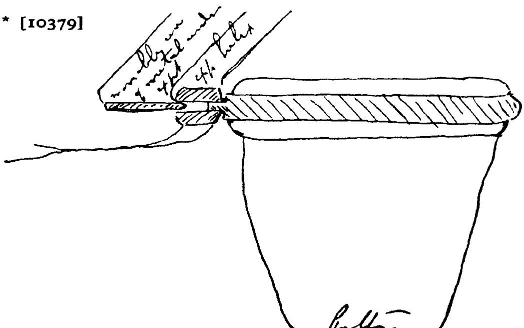
10379*. I have had one end of the iron core of the large cylinder Electro magnet (10330) turned smaller for about 1 inch in length, so as to fit the interior of the experimental helix (10189); the latter therefore is now fixed on to the former and tied to it, so as to make the two fixed in relation to each other. The hollow part of the experimental helix is of course the space through which the experimental core of bismuth or other metal is to travel. Now the Electro magnet and exp. helix make one thing and are supported on a steady stool, quite independant of the table on which the moving machine stands—or the exp. core which the machine carries. So that any communication of motion which may disturb the relative position of the Electro magnet and the Exp. helix is cut off. The experimental core is slung as before (10336) by the triangle and copper wire.

10380. The machine axes have been cleaned and oiled. The Electro magnet is now excited by 5 pr. of Grove plates well

* [10379]



charged, so that the magnet is powerful. All acts well. But there is this precaution requisite. It is not enough to make the Electro-magnet connection with the battery and then go to the Galvanometer and complete its communication, for if this be done at once the latter will be affected, and it will be affected for as much as 3, 4 or 5 seconds of time after connexion is made. At first I thought this was due to some current communicated from the battery to the helix, but found by investigation it was due to the gradual rise of the magnet to its maximum state, which occupying that time, could *during it* or *any portion of it*, induce on the helix and produce a current in it. Waiting a sufficient time and then making connexion with the Galvanometer, there is no effect on it produced, shewing that then the Electromagnet and the attached experimental helix are in the proper state.

10381. I *thought* that connecting the wires of the exp. helix with each other, so as to allow a current through it, shortened the time requisite for the Electro magnet to attain its maximum state of force. There may be some assisting action in a *surrounding* and *discharging* spiral, as respects the forces, that is worth examining—it may have connexion with the *tonic state*.

10382. i. *Copper core, solid*, in machine. Connexions all good and the working unexceptionable. The connexions always made one way at the Galvanometer so as to give a fixed and standard condition of them, and the index end of needle always spoken of as going *East* or *West*, according to the result of the experiment. When the machine was wrought, the needle end went well *West* (10348, 10371, 10427).

10383. i. A piece of *iron wire*, not magnetic, laid along the copper core and the machine again worked: the needle end went *East*, and that powerfully (10449). Hence iron or Magnetic indication is East. Induction indication is *West*, and Diamagnetic indication, if there be such a thing, should be also *West* ( ).

10384. i. *Copper core, solid.* After an hour and many other experiments, repeated the results with this core and the deflexion was again, as before, *West*, and to such a degree that the needle could be *held* 20° in that direction.

10385. i. *Copper filings core.* Expecting that division of the copper core into small particles would destroy this action by

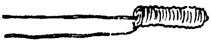
preventing the production of induced currents (10357), i.e. upon the assumption that the effect has nothing to do with the supposed *diamagnetic* polarity, I filled a thin glass tube, the size of the copper core cylinder (10384, 10386), with copper filings, and used this as the core. The needle end went *East.* After an hour and many other experiments, repeated the experiment with the same result. This effect is probably due to iron present, perhaps derived from the file used in producing them—or from filings accidentally present. For the copper power being gone by division, a very little iron would produce the effect obtained. Must clear the filings of Iron by digestion in dilute Sulphuric acid (10430).

10386. i. *Copper* WIRE *core* (10429). Have made up a copper cylinder of the core size ($5\frac{1}{2}$ inches long and 0·7 of an inch in diameter) by bundling together lengths (of $5\frac{1}{2}$ inches) of copper wire, which had previously been roughly cleaned by sand paper. This core therefore presents the copper so divided (i.e. axially) as to interfere with the currents produced in the mass by induction (for the plane of these currents is perpendicular to the lengths of wire and therefore to the direction of the division); but such division would not at all interfere with diamagnetic action, which if polar and antithetical to magnetic action, must be represented by currents round the *particles* of the copper. On working the machine there was *no effect* at the Galvanometer. After an hour and other experiments, repeated this trial, and again had no result as to Westerly deviation. When the working was sustained and the commutator at the Galvanometer brought into action, a trace of *East* deviation was obtained, probably due to the presence of a trace of iron. Indeed the lengths of copper had been cut by iron plyers. The copper however has lost all its power by division in this direction, which shews that its former effect when solid (10384) is not diamagnetic but due to the temporary induced currents in the mass (10429).

10387. i. *Copper disc core.* Another copper core was prepared by packing together 68 discs of thin copper plate—the cylinder thus produced was only 1·3 inches long and the rest of the $5\frac{1}{2}$ inches (10386) was made up of wood, to complete the attachment to the cap which, in the machine, lays hold of these cores and keeps them in position and connexion. Here therefore the copper was divided

as much as before (10386), but the direction of the division was parallel to the plane of the induced currents and therefore would not interfere with them or their production as in the mass. When the machine was worked, the needle end went with excellent effect to the *West* as with Solid copper. After an hour and other experiments, repeated the trial and found the needle end still go *West*, and though the cylinder was so short, the needle could be held 15° to the West steadily. Very good result and clear in its indications (10428).

10388. i. *Copper helix core, solid* (10436). This core consists of a covered copper wire wound into a solid helix, i.e. about a foot at one end of the wire used is left straight and all the rest is coiled tight upon this part as axis, the two ends coming away from the helix side by side. Now the spirals in this core are in the *plane* and *direction* of the electric currents that tend to be induced in the helix as a mass of copper, and in that respect they present no obstruction to the currents except that, if the ends of the wire be not in connexion, no current can exist in it and therefore none in the spirals, whereas if the ends be connected, then currents can exist in the wire as an endless wire and therefore in the spirals also. Again, if the ends be unclosed, so that no currents could be induced in the spirals of the *helix core*, then neither should any current be induced in the experimental helix (10379, 10389); but on closing the ends, currents should be induced in the *helix core* and in the experimental helix also (10389).



10389. i. First the *solid helix of copper wire* was used as a core with its ends unconnected together. There was *no effect* at the Galvanometer (10388). Then the ends were connected with each other and now the Galvanometer needle was well affected, the end going *West* as with solid core (10384). The ends were then opened out and the action disappeared; but on closing them it reappeared in full force. Very beautiful results (10686).

10390. Referring to the *helix core itself*. There is of course no current in it when its ends are not connected; but when its ends are connected, there is, as is indeed shewn by the secondary currents then produced in the experimental helix. But in order to confirm these points by experimental reference to the currents in the helix core, it was connected *directly* with the Galvanometer

and then it was easily seen that as the helix core entered the experimental helix, or rather as it approached the pole of the electro-magnet, a current was produced in it in one direction and as it receeded from the Magnetic pole, a current was produced in the other direction (10444, 10686).

10391. These results with copper divided in different directions are full of interest and instruction.

10392. i. *Lead, solid core.* Cylinder as before, $5\frac{1}{2}$ inches long and 0·7 in diameter, made of common lead and without care. The needle end goes *East* as if iron were in the lead and that is probably the case (10417).

10393. i. *Lead, disc core.* Discs cut out of sheet tea lead, not cleaned any way with care—and cut by a steel wadding punch. The cylinder formed in a tube was $5\frac{1}{2}$ long and 0·6 in diameter as to metal. On working the machine the effect was doubtful, being either none or needle end a little to the *East*, as if a trace of iron present. The difference between this and the former seems to shew that the former lead contains iron (10392) and that this is really purer lead (10419).

10394. i. *Lead, rolled core.* Some of the same tea lead was selected and cut into sheets $5\frac{1}{2}$ inches wide and then these were rolled up until a cylinder had been produced 0·7 inches in diameter. Here of course the construction was parallel to that of the *solid copper wire helix core* (10389), except that the ends of the spiral plate could not be connected. This core gave some very feeble indeterminate movements at the galvanometer—perhaps a little East. Therefore there was very little iron effect here, or if there was, it and the induced effect may have been mingled; but as the spiral roll could have had very little communication by contact of its coils, so the peculiar effect must have been nearly if not altogether 0°. Lead is a bad conductor.

10395. i. *Tin, solid core, cast,* ordinary tin. Needle end goes *West* very steadily and the result is, I believe, a fair good case of the currents by induction as in copper—not a case of diamagnetic action (10424, 5).

10396. i. *Tin, disc core.* Made up from discs of tin foil cut out by a steel punch. The core (metal part) is only about 2 inches long and contains about 730 discs. The Galvanometer needle went

well *West* and about as well as with the solid core (10395). The division into discs, whilst it does not prevent, does not I think *assist*, the effect. It is not quite as in the case of revulsion, for there the division into discs assists wonderfully, but then in that case the position and the plane of the discs therefore is oblique; here it is always perpendicular to the magnetic axis (10426).

10397. i. *Bismuth, cast core, solid* (10339, 10350). No effect—as before. When the machine was driven hard, the needle end even went East, but then I felt a little tap at the experimental helix and so that probably the cause (10416).

10398. i. *Antimony, cast core, solid.* Needle goes distinctly to the *West*—far better than my bismuth. The Antimony is only common Antimony and there is no doubt much iron is impurity in it, which must oppose this West tendency. Now to see if this is Diamagnetic or only inductive current action—break up part of the core into powder and try effect then (10415).

10399. i. *Platina core.* A square block of platina two inches long and 0·6 of an inch in the side was mounted as core and used. The effect was almost nil. Perhaps the least degree to the West. Should be East as a magnetic body and *West* as a mass in which currents could be induced (10423).

10400. i. *Green Glass tube* (10366). As before. No effect.

10401. i. *Sulphate of Iron.* A thin glass tube filled with small clean crystals of *proto sulphate of iron.* Could obtain no action at the needle whether the crystals were as a core worked quickly or slowly. Curious to see how little able this process is to detect Magnetic force and therefore also diamagnetic force. Surely the deflection of a needle a far more sensible test of diamagnetic condition of bismuth, if it had such a power (10367).

10402. i. *Phosphorus*, a good core in Glass tube. No effect. Really nothing, whether motion quick or slow. If it could be shewn by this process or on this principle, diamagnetic effect ought to have appeared here as well as if bismuth had been employed for the purpose, supposing bismuth polar diamagnetically.

10403. i, *Crystallized bismuth.* A group of crystals of bismuth weighing   grains which pointed Magnecrystallically between the poles of the magnet, was mounted so that the Magnecrystallic

axis should coincide with the axis of the experimental helix (10379). When the machine was worked, there was absolutely no effect at the galvanometer (10431).

10404. i. *Sulphate of Iron* (10434). Two large crystals of proto sulphate of iron were ground down into cylinders about 0·7 of an inch in diameter and these, being joined together and mounted as a core, gave a cylinder of the salt inches long, of which the Magnecrystallic axis coincided with the axis of the experimental helix (10379). No effect could be obtained. In this case the *Magnetic effect*, if any, should have been at a maximum.

10405. i. *Red ferro prussiate of potassa.* A fine crystal of this salt, full two inches long and above 0·8 of an inch thick, was mounted as a core and worked by the machine in the helix. The needle end went distinctly *West.* But the thickness of this crystal is too much, and I am not *sure* it does not now and then rub against the inside of the experimental helix, in which case error may have creapt in. When this crystal is in the Magnetic field, its length goes equatorially, which as Plucker has shewn is a Magneto optic effect, though it coincides with the diamagnetic position. It would therefore be a very important point to find whether it really has this power of sending the needle west, for though the effect would not be diamagnetic, it would be equally important as a magneto optic result—whether that were polar or not—permanent or transient. Thes[e] things would have to be enquired into. For this purpose I must reduce the crystal in width (10432).

10406. If Weber, by his pure bismuth and finer reflecting apparatus, obtained the effect he describes, it is almost certain that his result was the same in its nature as mine and not a case of diamagnetic action. The effect would of course be low in bismuth, because of its low conducting power, and the consequent feebleness of the currents induced in it. This shew[s] again in the revolving plate of Arago when made of bismuth.

10407. If the bismuth were powdered, then this induced current would be altogether stopped—though the diamagnetic action (if any) would in no way be interfered with.

10408. These results of inductive currents and the results of revulsion must be proportionate to each other, having indeed one common principle of action and excitement (10351).

10409. The Galvanometer contacts, etc. were tried at the end of the experiments by a thermo electric current and found all right. So they had been right through all the experiments.

10410. As to the second current, when copper core *in*, ↓, and stopped, ↑; or *out*, ↓, and *stopped*, ↑ (10359): if so, *in and stopped* should produce no effect, for ↓↑ = 0, and out and stopped should produce no effect, for ↑↓ = 0. But this probably is not so, for consider the case of the *solid copper helix*, when connected with the Galvanometer. If its entering produce in it one current and its stopping another, then putting it in and leaving it there should not affect Galvanometer, and taking it out and keeping it away should do nothing, and so putting in and taking out could produce nothing. I think the latter is the case, but I think that putting in and stopping gives one current and taking out and stopping the other (10390, 10437).

10411. But then should not this formation of *a* current and then its stopping in the core helix produce *two* currents in opposite directions in the experimental helix? And should not these neutralize each other? If so, the *in* of the *core helix* should produce *a* current in it but no resultant current in the experimental helix, and the *out* of the *core helix* should do the same thing (10389).

10412. Thought of placing a fine iron wire in the center of the copper core, reducing the wire in diameter until it and the copper were equal in effect in the contrary direction, and then weighing the two and so having an expression of their relative force. But if things were balanced this way, shortening both together would probably reduce the power of the iron faster than that of the copper, and then the copper would beat the iron. If this expected result should prove to be real, it might help to shew a distinction between polarity and non polarity, and so help to test the assumed polarity of diamagnetic action.

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10413. Adjusted the Machine—relieved the friction of parts, as of the commutator, etc. Applied 5 pr. of Grove's plates to the cylinder magnet to excite it, and finding the copper gave East deflexion, found that was due to *position of the battery*. So turned it end for end and now copper gave Western deflexion with the

standard contacts (10382), as before. By a thermo current found the connexions were good and right.

10414. The machine requires, as regards the Magnet and its experimental helix, almost an astronomical steadiness of fixation, because of the momentum of the rapidly moving cores and the vibrations it communicates even through the floor of such a place as the lecture room. It is not difficult to test the presence of any such source of error, and to do so in the present case, a glass core was fixed on to the end of the motion rod in the usual position, but in order to give weight and momentum to the whole, a leaden core was fastened to the hinder part of the rod. In this case, when the machine was worked, however long or quickly, there was no effect at the Galvanometer—until at last an accidental rub of the glass core within the helix instantly made itself sensible by a *momentary* effect at the Galvanometer. But both the cause and the effect are easy to be distinguished from the cause and effect when a copper core is used in the experimental helix.

10415. i. *Antimony core, solid* (10398). The former core had been broken in half, and the one half fitted up upon a wooden prolongation as a solid core and the other half pulverized and put into a glass tube (   ) as an *Antimony powder* or *divided core.* In two experiments at different times, the solid core gave a trace—a mere trace of *West effect.* But the *powder antimony core* in two different experiments produced no effect, either *West or East.* So antimony produces the inductive phenomena feebly, if solid; when pulverised, that effect is destroyed. The crystallization of the solid core was arranged from the center line to the circumference of the cylinder, as was to be expected. It probably had no influencing result over the nature of the action.

10416. i. *Bismuth, solid core* (10339, 50, 97). *No effect.* After a time, tried again. *No effect.* After an hour or two and with a new charge of battery to the Magnet—*no effect,* though the machine was worked well and quickly. Again tried but *no effect.* After some other experiments, again tried, but still *no effect.* Amongst the metals, Bismuth is the lowest of all that I have tried. Still, I dare say the currents are produced—though very feebly. Try by *revulsion* of some plates of bismuth (10714, 5, 6, 7[1]).

[1] Pars. 10704, 5, 6, 7. See note on p. 269.

10417. i. *Lead, solid core* (10392). The old core. Needle end went moderately and *fairly* WEST, as before. The effect comes on gradually, as is to be expected from the nature of the experiment, for the current which passes through the galvanometer is an intermitting one and of course very feeble and requires time to set the needle in motion. After other experiments, repeated this with the lead and again obtained a fair and true result to the *West*.

10418. I have made two cores from the lead of a tea chest, one smaller in diameter and longer than the other. Neither give me any effect—perhaps they contain a little iron (10450, 1).

10419. i. *Lead disc core* (10393). Needle went West. After a time tried again, and again went *West*, but feebly. Battery is now weak.

10420. i. *Gold core, discs*. This core was made up of 30 half sovereigns, well washed and enclosed as a cylinder in paper and prolonged by a stem of wood. The metal cylinder was about 1·2 inches long and 0·7 or 0·8 of an inch in diameter. It at once gave a good deflexion to the *West*, though not so good as copper. The gold is of course an alloy with copper and probably has its conducting power much affected by this, i.e. diminished. After an hour or two, tried again with a fresh battery to the Electro-magnet, and again gave *West*, and better than before.

10421. Here a case of division perpendicular to the axis not interfering with or at least preventing the result.

10422. i. *Silver core, discs*. 30 Sixpences well washed and made up into a core as above. Gave excellent *West* deflexion, 25° or 30°. After some time, tried again and with same result. Silver is as good if not better than copper and much better than the Gold. Is a half sovereign magnetic or diamagnetic?

10423. i. *Platinum, solid core* (10399). The ingot before described has been digested in hot dilute Sulc. and Mur. acid, with a little N. Acid added at the last to remove all externally attached iron. When employed as a *core* in the machine, it gave no effect either *East* or *West*.

10424. i. *Tin*, old solid core (10395). No sensible effect, but battery is much weakened now. Again no sensible effect, same battery. New battery and now it gave *West effect*, but moderate—as before and as is to be expected from its conducting power.

10425. i. Two cores have been made from Chinese tin, i.e. from the tin plate which constitutes the upper or top layer or side of the lining of a chest of *best tea*. These both gave *West effect* moderately and about as the former core. Whether this tin is originally Chinese or English I cannot say.

10426. i. *Tin disc core* (10396). The needle end went West–less than with solid tin, but very distinctly and fairly, especially by an alternation or two of the commutator at the Galvanometer.

10427. i. *Copper, solid core* (10382, 10448). Went *West* up to 30° or 40° with one contact at Galvanometer and fresh battery. After some hours, tried again. Well *West* as before. Again–effect well *West*. After other experiment tried again and was *West* as before. After another interval, again *West* with very moderate working at the machine. Results are very clear and good. Again another trial–*West*.

10428. i. *Copper* DISC *core* (10387). The needle end went as before *West* and fairly, but the battery was weak and the power not enough to shew full effect. Still, the non-interference of division in this direction evident.

10429. i. *Copper* WIRE *core* (10386). Here not a trace of effect. The division now stops all results.

10430. i. *Copper* FILINGS *core* (10385). These filings have been digested in hot dilute Sulphuric acid to remove all external iron derived from the files used, and being replaced in the glass tube, were now examined as a core divided in every direction. They gave no trace of action. The copper is apparently clear from that iron which before made it give *East effect* (10385), and now shews the result of division by its producing no *West* effect. Ever[y] thing else is in perfect order as to Battery, Magnet and connexion.

10431. i. *Bismuth crystal core*. No effect (10403).

10432. i. *Red ferro prussiate of potassa* (10405, 10452). This crystal has been rubbed down at the sides so as to make it of a right diameter, and now it certainly works in the experimental helix without rubbing. Now also I obtain NO deflexion of the needle at the Galvanometer.

10433. i. *Calcareous Spar*. A rhomboid of calcareous spar (not clear as Iceland spar) was rubbed into a cylindrical core, the optic axis of the piece being coincident with the axis of the cylinder.

The cylinder was of the width of the former cores (0·7 of an inch) and one inch long. If it gave any effect I thought it might give a diamagnetic or *West* effect, but the result was *nil.*

10434. i. *Sulphate of iron* core (10404, 53). The two crystals as before were tried again and I certainly obtained a small effect to the *East* as with iron or a magnetic body. I believe the effect was really true. It may be a conjunction of the action of the body as *a magnetic body* with its action as a body having the Magnecrystallic axis parallel to the axis of the Magnet and the experimental core. Must have another core with the M.C. axis perpendicular to this direction (10454).

10435. i. The tube of *small crystals of Sul. Iron* (10367, 455) produces an East effect. This distinct but very small. It in a certain degree answers the above enquiry.

10436. i. *Copper wire helix core, solid.* I have made such a helix of fine copper wire, covered. The wire itself is about the $\frac{1}{40}$ of an inch in diameter and 60 feet of it are made into a cylinder 0·65 of an inch in diameter and 2 inches long. The former helix (10388) consisted of wire $\frac{1}{24}$ of an inch in diameter and there was only about 20 feet of it in a cylinder 0·6 in diameter and $3\frac{1}{4}$ inches long. When the *fine wire helix core*, with its ends OPEN so that no current could occur in it, was used, then there was *no effect* upon the experimental helix or at the galvanometer. When the ends were joined together, then there was effect and to the *West* at the Galvanometer, though not as strong as before (10388). When the former helix of thicker wire was again employed, then the ends being closed, there was *West* deflexion at the Galvanometer and greater than with the fine wire helix. But when the ends were *open*, there was *no* effect. Excellent.

10437. The currents in the experimental helix are four for a cycle of motion (10359), thus, in $\uparrow -_{\wedge}$ stop $\downarrow -$ out $\downarrow -_{\dot{\wedge}}$ stop $\uparrow$; and the commutator has been arranged so as to change direction at the end of *in*, *before stopping*, and at the end of *out*, *before stopping*, as at $\wedge$ and $\dot{\wedge}$. But if the effect of stopping were connected with the effect of the preceeding and not of the following motion, then the result would be $\uparrow\downarrow$ and $\downarrow\uparrow$, and not $\downarrow\downarrow$ and $\uparrow\uparrow$. Consequently in the first case no final current should be produced, whereas in the last case they are produced and gathered up. Rearranged the commu-

tator so that the first instant of motion changed it over and not the last instant, as has till now been the case, so that the motion and the stop after it were combined in effect together (condition ii, 10463). ii
With this arrangement, introduced the copper core into the machine and experimented in every thing else as in all the former cases, but now not a trace of effect at the Galvanometer could be perceived. So putting in and stopping does *not* give a current in the Experimental helix, or taking out and stopping (10410, 1, 63).

10438. So the action of the Iron and of the copper is essentially different in their mode and results. Iron *in* and *stopped* will produce a current in the experimental helix. Copper will not. The same condition of the Commutator will give a maximum of the inducing effect of Iron and a minimum or 0 for copper. A body which is diamagnetically polar should give its maximum effect when the commutator is as for iron—but in the contrary direction.

10439. If the commutator change direction at the moment of middle passage, then the *half in*, the *stop* and the *half out* will be iii
one result and the *half out*, *stop* and *half in* will be the other result. Now the half in and half out will neutralize each other, and the consequent effect will be to compare the two stops with each other. These with Iron should produce nothing, for they are ineffective in producing currents, but with a copper core they should produce currents and in *contrary directions*. Hence a method of eliminating the effect of the stops with copper, etc. and of contrasting the iron and copper action, or the magnetic and inductive current action. Hence also a method of raising any polar Diamagnetic effect to a higher or lower degree and of discriminating it from the inductive current effect (Condition iii, 10458, 63).

10440. If a copper core had an iron wire axis, I ought to be able to separate the effect of the two. For if commutator changed *after* rest, it would give the iron effect, and if it changed in middle of motion, it would give the copper *rest* effect. Try all this.

10441. If all this be well founded, then surely a copper core and a weak *magnetic* core must produce different states of the Magnetic field for different corresponding instants of the time occupied in a cycle of change.

10442. Will anything arise out of these results or considerations bearing upon the effect of a central piece of iron in making a bar

of bismuth set more rapidly equatorially in the magnetic field? Or is that only a consequence of the rapid diminution of intensity of the lines of magnetic force when the iron is there? How would a *ball* of bismuth move between the poles, when the iron is there and also when away?

10443. Will any thing arise in relation to the difference in the law of increase and decrease of force on Magnetic and diamagnetic bodies.

10444. When the *wire helix core* (10436, 10390) was itself connected with the Galvanometer, then *in* and *stopping* conjoined gave one current in the core itself, and *out and stopping* conjoined gave the contrary current in the core itself. But when the core was both in and out conjoined, then there was *no* resulting current, as was to be expected. In fact, the Electromagnet acting on the *wire core helix* gives the results of iron or magnetic induction, and the wire core helix with its ends joined together gives in the experimental helix the effects of the copper induction. The two cases are here placed side by side.

10445. Endeavoured to ascertain whether much time was now occupied by the Electromagnet in rising to its maximum condition (10467, 511). Found that on making battery contact, I might immediately place the ends of the experimental helix in connexion with the Galvanometer without producing any effect there. If I was very quick, I could obtain a trace of action. I reversed the poles of the battery in relation to the core and still found this to be the case. So it appears that with an electric current from *five* pair of Grove's plates, this sized core, etc. is brought to a maximum state of Magnetism almost instantly. It was not so with 2 weaker pair of plates (   ). The point shews a precaution necessary in the experiments and the means of making it effectual (10577, 653).

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10446. Have set up apparatus in the Laboratory, the machine being on the lecture table. All seems steady. Both commutators are in place and arranged so that either one or the other may be used. The exciting battery is 5 pr. of Grove's plates. When the

Experimental helix is pushed quite up to the shoulder of the iron core (10379), there is always a current (due to derivation from the chief current) in it, which appears at the galvanometer; it was therefore withdrawn so far as not to be thus affected.
10447. The commutator is so arranged as to change just before the stop (10360), so that *stop* + *in* and *stop* + *out* are the portions of the cycle which it gathers up and sends on to the Galvanometer. This is the state in which the commutator was for all the former series of experiments (10360) and may be called *condition* i (10437, 10463).
10448. i. *Copper, solid core.* Some source of error, but slight apparently, the needle going East now and then 4° or 5°. But when the machine was fairly worked, the needle end went West as on former occasions (10371, 10382, 10427, 69), 40° or 50°. Hence the connexion appears to be good and in its standard condition.
10449. i. *Iron wire core,* being a soft iron wire in the center of a core of wood. Gave strong East or magnetic deflexion. All seems right (10383, 476).
10450. i. *Chinese lead,* thick short core (10418, 70). Needle end west.
10451. i. Chinese lead, longer narrower core (10418, 71). Will scarcely act: little impulses to and fro as if opposite sources of error in slight degree.
10452. i. *Red ferro prussiate potassa* (10432, 72). Apparently effect to the *West.* Appears as if there were something here.
10453. i. *Sul. Iron* (10404, 34, 73). *M.C. Axis axially.* Needle goes to the East fairly, as iron, but not so well as copper to the West. Still, the iron salt appears to shew itself *magnetic.*
10454. i. *Sul. Iron group. M.C. axis* ACROSS the cylinder into which the group is formed (10434, 74). Needle goes well East, same as in the former case.
10455. i. *Sul. Iron—crystals in tube* (10367, 435, 75). Needle end goes East and well. So all the iron salt acts magnetically.
10456. i. *Red ferro pruss. potash* again (10452, 72). Goes distinctly East now and I think all rubbing is avoided. Still, I cannot help suspecting some interfering source of Error.
10457. Arranged the commutator so as to change after stop, so

that *in* + *stop* would be gathered as one, and *out* + *stop* as the other constituent currents passing to the Galvanometer. This is to be considered condition ii (10437, 63); but it did not act well, for there were slips and irregularities and therefore I cancel all the results both with Iron and copper.

10458. Now put this commutator out of use and brought in the second, by which the *in* or the *out* can be divided into two, the commutator changing in the middle of a journey. This it does without fail, so of the two constituent currents sent to the Galvanometer, one is due to $\frac{1}{2}$ *in* + *stop* + $\frac{1}{2}$ *out* and the other to $\frac{1}{2}$ *out* + *stop* + $\frac{1}{2}$ *in* (10439), in which case the halves in and out should neutralize each other, and the effects of the stops only appear. This I will call condition iii of the Galvanometer (10463).

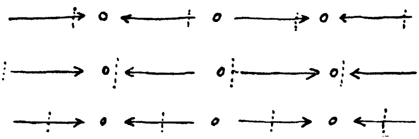
10459. iii. *Iron wire core* (10449, 500, 1, 12) iii. *In and stop*: produced an action at the needle which stopped suddenly, stopping the needle swing by a counter effect. *Out and stop*: an action, stopped suddenly; and the effect of both *in* and *out* was such as to leave little residual action. When the machine was worked slowly, one can see at the needle these short, tremulous, stiff actions and they very nearly neutralize each other, leaving however a little final effect to the *East*. The iron result is almost neutral, as from theory I expected it to be (10439).

10460. iii. *Copper*, *Solid core* (10448, 502, 15) iii. Had a good steady deflexion to the West (the reverse of the iron effect). This effect I supposed to be due to the sum of the two stop effects. Again: the same result. Again: a little irregularity now and then but the final effect to the West as before. Again: West. Again: *West*.

10461. iii. *Bismuth*, *solid core* (10506, 16) iii. Gave effects both East and West, and very irregular. Now think I have discovered the cause of this irregularity in the vibration of the table, which being surrounded by an iron railing, causes it also to vibrate isochronously with the motion of the Commutator, and as it is within 15 inches of the end of the Electromagnet, probably disturbes its action regularly and produces these interfering results. Must remove it or else take the apparatus to another place.

10462. Astonishing how great the precautions that are needed in these delicate experiments. Patience. Patience.

10463. Commutator conditions (   )



i or *stop* + *out* and *stop* + *in*

ii or *in* + *stop* and *out* + *stop*

iii or ½ *in* + *stop* + ½ *out* and ½ *out* + *stop* + ½ *in*.

iv

v

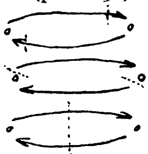
vi

Or (p. 2103[1])

i

ii

iii



These arrows simply indicate the direction of mechanical motion: not that of the resulting current.

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10464. Apparatus rearranged in the Laboratory—the machine being at Pneumatic trough place on a table well strutted up—the Electro magnet and battery and Galvanometer on separate tables on the stone floor and the latter so far from the magnet as not to be disturbed by it. The Commutator was in the *Condition* i (10463) and the connexions such as to give the former deflection, i.e. for iron to and fro, *East* deflection, and for Copper to and fro, *West* deflection. The experimental helix was of course upon the iron core of the Electro magnet, but it was separated from contact with the coil of the E.M. by interposed Gutta Percha, and the ties which held it on were of silk thread, to prevent any current passing to it from the battery—the latter was 5 pr. Grove's plates.

10465. Making the Electro Magnet active deflected the Galvanometer needle perhaps ½ a degree West by mere Magnetic action—but after waiting 2 or 3 seconds and then completing connexion of the wires, the Needle end went *East* and stood there—going back when contact was broken as if effect was from a *derived current*. Then the copper solid core was put into its place, being

[1] I.e. par. 10537 *et seq.*

out of the helix–but the same result occurd. When the core was in the Exp. helix, then the result occurd., but 3° West, the commutator having passed over the change position. So that the copper core does nothing. Then unconnected the battery and the Electro magnet and now there was no such current.

10466. So this current is not due to any action here or there in the course of the Galvanometer connexions, but must be derived from the magnet or its exciting current by the exp. helix. Yet the iron core seems to be the only place where such communication could occur as could produce it. I think it cannot be a current of induction either from the magnet or its coil, but must be derived some how or other, i.e. gathered up and off from the principal current. Must clear up the effect hereafter–but make allowance now.

10467. In another part of the day, with a fresh battery, I had after making magnet contact that current for a while which seems to be due to the iron core gradually rising to its maximum condition (10445, 511, 77); which rise to the maximum condition seems to be quick and sudden when the magnet is frequently made and unmade in succession, but slower if a longer interval of several hours or days intervene.

10468. Now experiments with the commutator in condition i (10463), the interval between the ends of the experimental cores and the iron of the electro magnet, when nearest together, being about the $\frac{1}{5}$th of an inch.

10469. *Copper, solid core*, i (10448). Gave good *West* effect.

10470. *Chinese solid lead*, short core, i (10450). West–feebly.

10471. *Do.* thinner, longer core, i (10451). West still more feebly. Shews that it is not the length of the core but the mass of the metal at the end in the helix and near the Magnet which tells.

10472. *Red ferro pruss. pot.*, i (10452, 56). A little irregular motion, very small and caused I think by rubbing against the Exp. helix. The true salt effect seemed to be nothing, either *East* or *West*.

10473. *Sul. Iron, M.C. axis axial*, i (10453). If any effect, it was a little to the East–but the core is too large in diameter and liable to rub.

10474. *Sul. Iron, M.C. axis transverse*, i (10454). If any thing, East.

10475. *Sul. Iron crystals in tube*, i (10455). If any thing, East.
10476. *Iron wire core*, i (10449). Very Strongly East.
10477. Now arranged the commutator so as to act under condition ii (10463) and beginning with the Iron wire core (10476) and using the Electro magnet *unexcited*, and therefore active only by the small amount of Magnetism which it can retain, the effects were as follows. The *in* or *to* of the iron wire gave a good Western deflexion—hence effect is distinct, but as I desired an East deflexion for iron, I changed the connexion at the Galvanometer and so produced and shall continue to produce that effect for iron in this series of experiments. The condition ii of the commutator ought to bring out *both Magnetic* and *diamagnetic effects* (if no result dependant on time be concerned), but give no *inductive effects* of the kind that I believe are produced in the copper and other cores.
10478. *Iron wire core*, ii (10477). East either with feeble residual magnet or the fully excited magnet.
10479. *Copper, solid core*, ii ( ). *Nothing*, either with feeble residual magnet or the fully excited magnet. Onwards only use the excited magnet.
10480. At this period, the sensibility of the Galvanometer needle was reduced considerably by an accidental inductive current sent thrgh. the Galvanometer whilst wires were connected. Still, I was able to obtain very good and distinctive results.
10481. *Copper, solid core*, ii ( ). Neither *East* nor *West*.
10482. *Lead, solid core*, ii ( ). Nothing. Made the Galvanometer more sensible—but still not up to the first condition (10480). Still the lead gave no effect.
10483. *Bismuth, solid core*, ii ( )—not a trace of action.
10484. *Antimony, solid core*, ii ( ). If anything—the least trace of East deflexion (perhaps from iron) but very doubtful.
10485. *Platinum*, ii ( ). Nothing.
10486. *Sul. Iron*, crystals in tube, ii ( ). Nothing.
10487. *Sul. Iron, M.C. axis transverse*, ii ( ). East—very little.
10488. *Sul. Iron M.C. axis* axial, ii ( ). East—very little.
10489. *Red ferro pruss. pot.*, ii ( ). East the least trace—if any thing.
10490. *Silver discs*, ii ( ). Nothing. Good.

10491. *Copper helix core* with ends connected together ( ), ii. Gave no effects in the Experimental helix, therefore results same as with the solid cores above. But when the ends of the copper helix core were connected with the Galvanometer, it gave a current and deflexion to the East. Thus shewing the current produce[d] in it, but which in this state of the commutator could not induce currents in the Experimental helix ( ).

10492. *Iron wire core* ( ) ii. Machine worked quickly; there was no effect or scarcely any. When worked more slowly, obtained unsteady results and found that the Commutator was deranged and occasionally out of contact, or rather slipped; by a little rosin powder created friction and rearranged it to work well, and now the iron wire core gave East effect for both states of commutator and so strong as to be hardly manageable. Renewed therefore some of the expts. above.

10493. *Sul. Iron crystals in tube* ( ) ii. East as iron.

10494. *Sul. Iron, M.C. axis axially* ( ) ii. No sensible action.

10495. *Sul. Iron, M.C. axis transverse* ( ) ii. No sensible action; probably needle not sensible enough for these two cores.

10496. *Iron wire* ( ) ii. East–good. Commutator right in action.

10497. *Red ferro pruss.* ( ) ii. Nothing.

10498. *Copper, solid* ( ) ii. Nothing.

10499. *Iron wire* ( ) ii. East–all right and commutator all right.

10500. Now used the second Commutator in the condition iii (10463), in which $\frac{1}{2}$ *to* + *stop* + $\frac{1}{2}$ *fro* makes one commutator current and $\frac{1}{2}$ *fro* + *stop* + $\frac{1}{2}$ *to* makes the second commutator current. Used at first the iron wire core and the Electro magnet without the battery in its residual state. If *iron core in*, iii (10459, 512) one motion, the needle end went West; if *out* one motion, the needle end went East; and the two seem nearly equal in force, for *in* and *out* combined leave the needle close upon 0°. The *in* motion of course gives two half currents or effects, its results being halved and opposed by the change of the commutator midway, and as the second half of the *in* is nearer to the magnet than the first half, there ought to be a surplus of action, which is that which makes

the needle end go *West*. The *fro* or *out* passage is also divided equal in time but unequally in force, the first half being the strongest, and that gives the surplus action which makes the needle end go *East*. As these two surpluses are equal in power *or ought to be*, so a *to* and *fro* leaves the needle at 0°.

10501. When the machine was worked *continuously* and slowly, then the needle was not left at 0° but creeped by a series of feeble jerks towards the *East*; increasing the velocity very gradually, this effect increased, then diminished, then with a certain velocity became nil, and with a higher velocity became *West* deflection, going up by creeps isochronous with the motion of the commutator. When the Electro magnet was excited, the same effects occurred except that the change from East to West occurred sooner (10507, 12).

10502. *Copper core* (10460, 515, 10640), iii. Needle end goes fairly West. Not strongly, but remember that it is only the effect of the *stops* that is gathered up now and therefore only half that which the copper core can produce.

10503. *Platina* (   ) iii. Nothing.

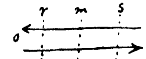
10504. *Bismuth* (10461, 516) iii. Nothing.

10505. *Antimony* (10517) iii. Nothing.

10506. *Bismuth* again (10461, 516) iii. Nothing.

10507. I now found that with this commutator in its present condition, the pull raised one tongue from its bearing at the end of the *out* or *fro* motion, and that therefore contact was broken at that time and during the *stop* after fro. This may have caused part of the iron action (10500, 1, 12), and may have varied in its extent with different velocities, so correct it and repeat the results. I do not see how it could alter the induction results in character, though it would of course weaken them much by taking off the effect [of] one *stop*. I have now set the commutator free, that it may bear on the lower plate for the whole time.

10508. Must vary condition iii by changing the place of inversion and placing it nearer to or further from the stop at each end, as for instance at *r* or *s* as well as at *m*.



10509. *Precautions*. The film of oxide which forms on the commutator plate in 12 hours is enough to insulate and therefore stop the passage of the current to the tongue above. Also the fine

binding screws in the moveable parts of the commutator are apt by the working of the machine to become loose and so cause failure of contact there. It is necessary continually to try the connexions thrgh. the circuit by a thermo electric current.

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10510. Apparatus in order. The second commutator in Condition iii, ready to move and rubbing constantly at the points. The connexion all right and by Iron thermo current standing 10° steadily to West in every position of the commutator. The Electro magnet prepared ready with 5 pr. Grove's plates. When the Experimental cores are in place and *in* or *to*, then an interval of $\frac{1}{5}$th of an inch is left between their ends and the end of the Electro magnet cylinder core towards which they face. When they are withdrawn, this distance is increased to 2·2 inches, but that end of the exp. core is all the time within the experimental helix.

10511. There being no exp. core on the machine and the Electro magnet not urged by the V. Battery, but only acting by its retained magnetism, when the connexion was made at the Galvanometer, all was quiet—there was no current in the experimental helix; none was expected. But first putting on the force of the battery and after a second or two of waiting, completing the connexion at the Galvanometer, there was a current produced, which lasted 10 or 15 seconds but gradually went down to 0°. Is not this due to the core of the Electromagnet occupying that time in rising to its maximum condition by the current used? On breaking the battery connexion and after a few seconds renewing it, the same effect was produced but for a shorter time. So the current is clearly not one derived directly from the battery used, but a temporary current which is greater according as the Magnet has had a longer rest (10415[1], 67, 10577). I think Marianini described these conditions of a magnet some years ago. After the magnet has been excited a few times—it rises up to its maximum so quickly that a single second is sufficient for the purpose. But here another *Precaution* to be taken.

10512. *Iron wire core* (10449, 59, 500, 1, 10631) iii. The magnet used unurged by the battery and in its residual state, consequently

[1] Query par. 10445.

weak only. *To* gave a double impulse on the needle, first east and then west, with an excess of *West* deflexion, so as to leave it moving a little that way; *fro* gave a strong East deflexion, suddenly diminishing much and leaving only a little East deflexion. The united effects of *to* and *fro* are equal or as nearly so as possible, for when the machine is worked moderately, the needle end is knocked backwds. and forwds. within very short limits and keeps within 1° of 0° on either side. When the Electro magnet was urged by the current, then all these effects were produced on a stronger scale. *In* left excess of West–out excess of East–and when the machine was worked, the needle wavered about 0°, having sometimes a little excess, 4° or 5° West or East, changing from one to the other according to small accidental result connected perhaps with the time of working or of the alteration of state.

10513. These effects are as might be expected. As the in motion occurs, the commutator divides the time and the journey into two equal halves, the effects of which are sent through the Galvanometer. But the half which is nearest to the end of the Electro magnet, i.e. in which the experimental core is nearest to it, is more efficacious in its conditions than the further half, and gives a stronger current. As the experimental core performs its journey in, it is the second which is the strongest, so we see first an East direction produced by the first half of the journey, and then that stopped by the second half of the journey and the needle sent West by the surplus force of this half. As the core passes out, the first half of the journey gives a current which sends it strongly East, but the second half of the journey produces a weaker current in the same direction in the exp. helix, but which, being by the change in the commutator opposed to the first half, suddenly causes it to fall greatly, leaving only that little degree of East motion which it was not able to neutralize. Further, when these two residual portions of force were pitted against each other by making a complete cycle of motion at once, then the needle was left where it first started, knocked about visibly but by blows on the opposite sides of equal force.

10514. Surely it will not be difficult to adjust the commutator so that the force of the two combined half journeys shall be equal

(10508, 634) and then the *to* and the *fro* will each leave the needle unaffected.

10515. *Copper core* (10460, 502) iii. Good. West effect about 10° or 15°. This is only the effect of the sum of the stops ( ).

10516. *Bismuth core* (10461, 506) iii. No trace of action–no induction in it because of bad conducting power. Of course diamagnetic effect would not have been shewn here, nor Magnetic effect. The connexions were tested by the thermo current and found to be good in every position of the Commutator.

10517. *Antimony* (10505) iii. No trace of action.

10518. *Lead* ( ) iii, a little West, as copper, 1° or 1½°.

10519. *Tin* ( ) iii, fairly West–about 3°.

10520. *Silver* (*sixpences*) ( ) iii. West, 10°.

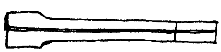
10521. *Lead discs* ( ) iii. West very feebly, 0·9° or 1°.

10522. *Copper discs* ( ) iii. West, about 5°. Connexions again tried by the Thermo electric circuit and found all good.

10523. *Copper, long wire core* ( ) iii. Nothing.

10524. *Copper filings* ( ) iii. Nothing.

10525. *Tin disc core* ( ) iii. West, about 1½°.



10526. I have a core of copper and wood; the copper end is a cylinder inches in length and the wood long enough to make the whole the length of the standard or copper core. An axial hole is made through the wood and copper, and a long wire either of copper or iron, which is tapped into the outer extremity of the copper core, holds the two together. When the iron wire is used, the core is compounded of copper and iron, and may in its action be considered as such. When the copper cylinder is taken away and the iron wire left, it represents the iron part of the core alone, and when the copper wire and cylinder is used the arrangement represents the copper part alone. On the whole, the attachment was too delicate, so that the copper and wood were not stiff enough together, but the following preliminary results were obtained.

10527. The iron wire (finest) with Electro magnet urged by the battery gave alone East deflexion as a residual action.

10528. The copper cylinder, held in place by copper wire, gave alone West result moderate from 5° to 8°.

10529. The copper cylinder held by the iron wire gave joggins

of the needle, but a residual result to the West far beyond the copper effect alone; it was 30° or more.

10530. The Magnet without the V. Battery was used and the needle end worked East under the result of the combined *iron and* copper. Again, Magnet urged by battery, the needle end went West 20° or 30° again. The copper was now taken off and the iron alone left to act, and now the needle end was to the East with jogging.

10531. So either the copper by its momentum pulls the iron to and fro and makes it do something *West*, or else the Iron makes the copper do more West than it can do alone. Must examine this with a better core.

10532. *Platina*, iii. West a little—but the core is square and too large to be safe in its action.

10533. *Sul. Iron*, *M.C. axis* transverse ( ) iii. Nothing.

10534. *Sul. Iron crystals in the tube* ( ) iii. Nothing. The connexions are all good by trial with thermo current.

10535. Copper helix ( ) iii, closed and used as core. Little or no trace of current from the Exp. helix at the Galvanometer. The helix itself was then connected with the Galvanometer through the commutator, and gave Very little effect, scarcely a trace *West*. This latter case of course should be so; because the commutator changes in the middle of the to and fro, and so each half cycle consists of two halves having contrary and equal forces ( ). But I expected a better result from the Experimental helix.

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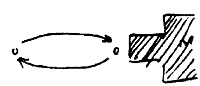
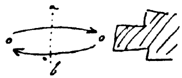
10536. The machine has been examined and the commutators cased and adjusted so as to work well I trust. A Battery of 5 pr. of plates is in use—the connexions are all perfect in all states of the commutators and the Galvanometer needles have been strengthened and equalized—so that the instrument is very delicate.

10537. The second commutator is in use and in the condition iii. In order to have an easy reference to these conditions, such a diagram as this may be used, where the upper arrow represents the passage or journey of the experimental core end *in* or *to*, and the lower arrow the passage *out* or *fro*, the two *oo* representing

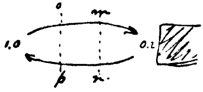


the respective stops at the change of motion. M represents the end of the Electro magnetic core, which I will always consider as on the right hand of the moving experimental core. Now in



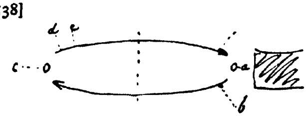
position iii, the commutator changes the direction of the currents that go to it in the middle of the journey, thus; and therefore picks up first the results of the action on the right hand of *a b*, and then the results of the action on the left hand, and so on continually. Whatever these are, they make the two currents, which may be called the commutator currents, and which it sends on to the Galvanometer in each complete cycle of motion *to* and *fro*.

10538. Iron wire core (   ) iii. Used the magnet in its residual state (10530), i.e. not excited by the battery. When the iron went *to*, there was the least trace of final motion of the Galvanometer needle end to the West; when *fro*, the least trace East. When to and fro successively but slowly, the needle was agitated slightly, but kept its place, shewing the equability of the contrary actions. When the machine moved more quickly, then the needle end creeped slowly a little to the *West*, as if the results of the *to* action were stronger than those of the *fro* action, with a quick velocity though not with a lower. Can this depend upon a difference in the time of receiving and losing magnetic state in the iron core? I should expect it would rise up more rapidly in the latter part of the *to* than it falls in the beginning of the fro, and that the fall continues longer in the latter part of the *fro*, than the rise in the beginning of the *to*. That the rise in power does not indeed cease with the cessation of *to* but continues for an instant during the *stop* there, and on the other hand, that the fall does not begin the very instant *fro* commences, but a moment after, and continues into the stop after fro, and if the motion be quick may even continue into the beginning of the following *to*. In which case, the maximum magnetism of the end of the iron core would not be at *a** but somewhere about *b*—and the minimum also would not be at *c* but perhaps at *d* or *e*. Follow this out in its effects hereafter (10565).



10539. I now altered the adjustment of the commutator so as to make the change occur at *o*, *p*. The time therefore during which the commutator gathered up the effects without change was much longer whilst the experimental core was in the vicinity of the

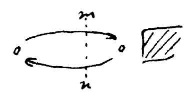
* [10538]



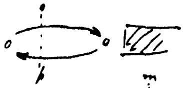
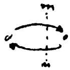
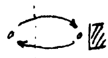
magnet than when it was withdrawn. The two parts of the cycle were unequal, one being (the in) about two thirds of the whole cycle and the other (the out) about one third; but of each of these parts, the *to and fro*, both as to time and space moved through, were exactly equal to each other. It is very clear that as regards the commutator action, when the machine works regularly, the commutator sends on to the Galvanometer the effects produced from *o* to stop 2 and round to *p* by one wire side, and that if these effects be contrary (as they will be), that the needle will shew them in succession and be left influenced by their difference *if there be any*; that then the commutator will change and send the rest of the fro current from *p* to *stop* 1 and the *to* current from stop 1 to *o* to the Galvanometer by the other wire side, and if the powers evolved in the cycle of change be equal, the needle will then be at rest. On the other hand, if a single *in* motion be made, then the needle will first shew the effect produced from stop 1 to *o*, and afterwards in a contrary direction (because of the change in the commutator) the effect produced from *o* to stop 2, and we have seen that these are not equal and were not likely to be equal (10538).

10540. With the commutator then as just described (10539) and the *in* period therefore the longest, and with iron wire core and residual state of magnet, when the machine was worked, the needle end scarcely moved either way, so equable are the forces evolved in the different parts of the cycle as thus divided.

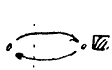
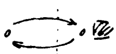
10541. But when the commutator was altered to *m*, *n*, so as to make the *in* period the shortest, then upon working the machine the end of the needle gathered up a little towards the *West*.

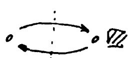
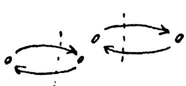


10542. The Magnet was now excited by the battery so as greatly to strengthen the Magnetism, but not to change its direction, and the commutator was left as above (10541) with the *in* period the shortest. When the iron wire core passed *to*, the needle end went *East*, and when it passed *fro*, it went West (which is the contrary of what occurred with the residual magnet and the commutator adjusted midway), but there was very little trace of motion either way. When the machine was worked slowly but continuously, the needle end kept a little to the East, but when the motion was quicker, it went to the West, being in this respect like the action of the same core and the residual magnet (10538).

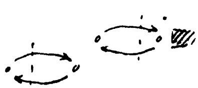


10543. Again made *in* period longest (10540) and used the battery to the magnet. When the machine moved slowly, the needle stood at 0°, but when more quickly it gathered up *West*. Made *in* period shortest and worked the machine; the needle again gathered West and I think a little quicker than before. So whether the galvanometer divides the journey midway or near stop 1 or stop 2, the needle end (standard) always gathers West with these connexions, but the forces evolved in the whole cycle seem nearest to neutrality when the *in period is longest*, or by commutator thus.


10544. Several repetitions of these experiments in the after part of the day gave the same result. My commutator does not allow me at present to divide the cycle near[er] to the stops than about $\frac{1}{3}$ of the whole distance of the journey.



10545. *Copper, solid core* (   ). Now worked with the long copper core and the commutator in condition iii, varied as before described. When the *in* period was longest, working the machine (magnet at full power) produced no effect at the Galvanometer. Yet here I expected to obtain at the Galvanometer the joint effects of the stops (10439). Then made the *in* period the shortest, and now obtained 40° West with one contact at Galvanometer. After a time, again with in period the shortest, obtained 40° West or more.

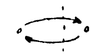


10546. Made commutator change midway (10537) and now by working the machine the needle end went West, but less than when *in* period short. Changed commutator and made in period the longest, and now again the effect was almost insensible, scarcely West. Changed the commutator to *in* shortest–the copper [? needle end] then went West but only feebly and not at all as I expected.

10547. It would appear therefore that when the *in* period is shortest, the best effect is obtained with the solid copper core. But I am jealous of the results, because of the apparent falling of[f] in power of the copper. I found also it had started from its place and I must repeat the above results unexceptionably.


10548. To compare in some degree a long and short core of copper, I had a compound core of wood and copper, the latter or copper only $1\frac{1}{2}$ inches long. With the *in* period short, the needle end went *West* as with the long copper. With the *in* period long, the needle end went *west*, but feebly only, as with long copper.

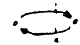
10549. Now used some other cores with the *in* period constantly short as that which gave best results.

10550. *Tin core* ( ) iii. Needle end *West*—very feebly.

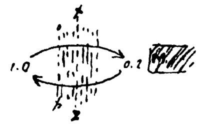


10551. *Silver discs* ( ) iii. Needle end *West*—nearly as copper.

10552. *Bismuth core* ( ) iii. No effect.



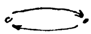
10553. The present commutator can divide the journey either at one third of the whole journey from either end, or at any point nearer the middle of the Journey. Wherever it divides, the sum of the actions on the one side, as from *o to stop* 2 *and on to p*, are sent on by one side of the galvanometer circuit to it, and then the sum of the actions on the other side, as from *p to stop* 1 *and on to o*, are sent to the Galvanometer in the contrary direction (10539).



10554. Now as the velocity is a maximum at the points (middle) x and z and gradually increases to and diminishes from these points to stop 1 and stop 2, so the latter places cannot be taken as giving the effects of the stops, for the effect of stop 2 is on even at x and is almost complete before *o*.2 is attained, especially in respect of the *time* of the journey; and so also the effect of stop 1 is on from z to 1.*o*. In fact, as regards velocity, the *to* and *fro* is from *o*.1 *to* x and from *o*.2 to z, and from x *to stop* 2 represents stop 2, and from z to stop 1 represents stop 1.

10555. But then, during the whole of the *to*, the core is entering parts of the magnetic field which are more and more intense, and of necessity is cutting magnetic curves which diverge equally in all directions from the point M of the Magnet, and it does not cease to cut these curves or really stop in effect until it ceases to advance; and the same is true for the *fro*, and it is really difficult to say where the virtual representation of the effect which may be considered as due to stop is to be placed. As a copper core advances, currents round its axis will be induced which, rising up from o, will attain a maximum intensity and then gradually diminish to o, to be succeeded by contrary currents which will gradually attain a maximum and then also diminish to o, to be succeeded by the first set again. Now from o to maximum will induce a current in the Exp. helix wire in a given direction, which may be represented thus ↑; and from maximum on to o will

produce a contrary current thus ↑; from the last o to the next maximum will produce the same current as the last ↓; and from that maxm. to the following o will produce this current ↑; so that the four currents will be as follows: ↑↓↓↑. Or if we take the cycle of effects with its two maxima and two minima, then from one maximum to the other will give one current on to the commutator ↓, and from the second maximum on to the first will give the other current ↑. These two currents it is which the commutator should pick up and send on to the Galvanometer, and it therefore should change sides at the maxima.

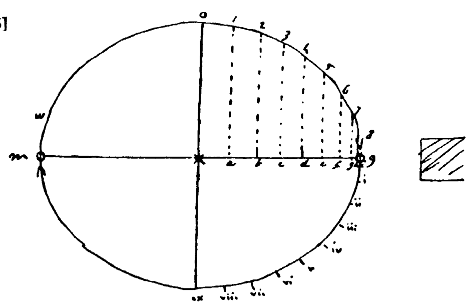


10556*. Now let the circle represent the to and fro enlarged and widened. As the core is carried to and fro by a crank wheel, of course the figures 1, 2, 3, 4, 5, 6, 7, 8, 9 represent equal times, and from ⊠ to *a*, from *a* to *b*, from *b* to *c* and so on represent also *equal times*. But then the distances between ⊠ and *a*; *a* and *b*; *b* and *c*; etc. shew the velocities for these equal times.

10557. If the maximum current (10555) depended upon velocity alone in the motion of the copper core, it would be greatest at o or ⊠, but as from there to 9 or *i* it is entering in a more intense part of the magnetic field, it must be somewhere on towds. this place. If the maximum current depended upon the most favourable place in the magnetic field, it would be at 9 or *i*; but depending upon the motion and its velocity, as these are nothing there, the current must be o there (except for any effect of time) and the maximum therefore of necessity somewhere towards o or ⊠. Perhaps I may assume it about 5 or *e*. Then the following minimum or o will be at 9 or *i* (except for time).

10558. On the fro or retreat of the core, the velocity will be greatest at ix, but that cannot be the place of the second maximum because the magnetic field is much weaker in intensity there than at viii, vii, vi, etc. Neither can it be between ix and *m*, for the field becomes weaker and the velocity less and less; and for the

* [10556]

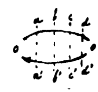


joint considerations above given, it is most probably somewhere about v. The second minimum or o will of course be at *m*.

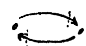
10559. If all this be right, the two maxima will be at 5 and v, and the two minima or o at 9 and *m*; and it is at the maxima or 5 and v that the Commutator has to change, which can be done best with the second commutator in Condition iii, thus (10655).

Must compare effects thus obtained with those obtained by the commutator condition i on former occasions.

10560. The motions *to* and *fro* must produce I think equal antagonistic effects in the parts which correspond, as at the parts *a a*′; *b b*′; *c c*′; *d d*′, for if *o* and *o* be the minimum or nil points of action, then the other parts must correspd. So if the maximum of one current be at *d*, the maximum of the other currrent will be at *d*′.



10561. I then made some experiments with the first commutator in condition i, using the solid copper core, but I obtained very poor effects indeed and was altogether confused in the results. Whether this is due to the exhaustion of the Voltaic battery or any other cause I do not know, but must clear the matter up.

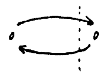


10562. Think a little about action of Iron wire core and where its maximum effect on the experimental helix would be. Excepting the influence of time occupied by its assumption and loss of the magnetic state by approximation and recession, it is clear that the minima of action or o, would be at *m* and 9, as with a copper core (10557). Now here the pole which acts on the helix rises in intensity all the way from *m* to 9 and is most intense at 9, but then it has no motion relative to the exp. helix there. The quickest motion is at ⊗, but then it is not strgest. in force there. The maximum effect due to these two causes will therefore probably be about 4 or 5, and perhaps is exactly where the maximum for the copper core is (10559), which is to me rather unexpected. Then on its return fro, the maximum effect would be at the correspondant iv or v, say v.

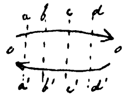
10563. But whilst considering the copper core (10556), the two maxima found were the maxima of the copper core condition, not the maxima of its action on the experimental helix; on the contrary, they were the minima of action on it and were the times when the commutator should change (10555). But with the iron,

the two maxima above (10562) are the maxima of action on the experimental helix; the corresponding minima and the times therefore when the commutator should change are at *m* and 9 (10556), and therefore very different to those for the copper core.

10564. Must test all this by experiment. As to the effect of time, it will probably be of such a nature as to make the maximum condition of the iron core occur about i or in that direction and its minimum condition perhaps at *w* (10556); and if so, these will be the places for the commutator to change. Now changing at these places should give almost o for the effect of the copper core on the helix. So

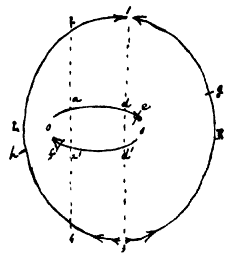
 will be best for copper and worst for iron;

 will be best for iron and worst for copper.



10565. As regards iron (exception being made for time effect), though the condition *d d* as here given is worst for iron, yet any other condition which gives equal portions of to and fro on the same side of the commutator change, as at *c c'*, or *b b'* or *a a'*, should give the same bad effect, because the equal portions of contrary action from *b for instance to stop and back to b'* will neutralize each other and so send no current on to the Galvanometer, and the same will be the case from *b' to o and on to b*. As regards copper, such a change will do harm, for if from *d to o and on to d'* gives one current, and then from *d' to o on to d* gives a contrary current, making the commutator change at *b b'* would be to add the effect from *b to d* and that from *d' to b'* together, and to take their sum from the current due to *d to o on to d'*; then the remainder left, and the current produced from *a' by o on to a*, would together make that which would pass on to the Galvanometer, and probably be only a small quantity.

10566. In reference to condition i and the results it has already given me, it would not seem to be quite so favourable for iron as condition ii, i.e. if the effect of time be sensible; if not, they will be nearly alike and almost the best. In respect to the effect upon copper, the one or the other would be a weakening, for if *d d'* represent the places of change (10555), then the two semicircular



arrows L and R may represent the two equal currents then gathered up in succession by the commutator. As already said, if the commutator change at *a a'*, then portions of the one current L equal to 1–2 and 3–4 are opposed to an equal quantity of the other current R and are lost, and the two remainders only go to the galvanometer (10565); but if the commutator divide at *d* and *a'*, then only half as much, or the one part from 3 to 4, neutralizes an equal portion of R, and therefore more goes to the Galvanometer than before. If the commutator change at *e* and *f*, then the part current from *g* to 3 and the part opposite current from 3 to *h* are united and only their difference remains; on the other side, *h* to 1, and 1 to *g* are opposed and only their difference remains; and these two differences make up all that go to the Galvanometer. When these vanish by making the change at *o* and *o*, then we have the worst condition for copper, as already said (10564).

10567. Whether time has any thing to do with the currents in copper I have not considered here, but from my explication of Arago's Phenomena in reply to Nobili, think it has.

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10568. Action of *iron ore*. The exp. helix is on the end of the Electro magnet core and the latter is used as a magnet in its residual state unexcited by any battery. The Exp. helix is connected with the Galvanometer and no commutator is in use. The connexions are perfect.

10569. The iron wire core is $5\frac{1}{4}$ inches in length (   ) and well annealed. Introduced by hand slowly into the exp. helix, a current in one direction was produced during the *whole time* of introduction of the core. A current in the contrary direction was produced during the *whole time* of withdrawal of the core. The stops at the end do nothing, and are as supposed (10562), the places of minimum action.

10570. Using the excited magnet, 5 pr. of plates and dividing the whole journey in into several equal parts and observing the current due to each; as these parts were nearer to the magnet so the current was more powerful. The first four fifths of the journey in did not produce more effect altogether than the last single fifth.

The effects fro were the same in kind; the first fifth out equalled the effect of the remaining four fifths. Hence the maximum place of action or the middle of the effect. current produced in the experimental helix is, as was supposed (10562), very near up to the inner end of the Journey *for iron.*

10571. The iron core was introduced slowly (time about 2″) until stopped by a thin blocking cork, and the extent of the swing of the needle end noted—it was 16°. Again, when all was at rest and in order, it was introduced exactly as [be]fore but quickly (time perhaps 0·1 of a second); the swing of the needle was again 16°. Every repetition of this experiment still shewed the same result. The needle swung on in its course long after the motion of the core in either case was over, occupying 6″ or 7″ or even more. So that the power in both cases was fully exerted upon it. Hence it appears that with Iron, whether the core moves up to the magnet quickly or slowly, the same amount of effect is produced on the Exp. Helix and galvanometer (10585, 6). When the core was removed quickly and slowly the same equality of effect was produced.

10572. The swing at the Galvanometer needle with the residual magnet seemed to be a trace stronger for a single *to* than for a single *fro*. Perhaps this may depend upon some point either of time or something else connected with a single action, for several *tos* and *fro* in succession left the needle at 0°.

10573. I expected that a short iron core would produce less effect than a long one, and that perhaps it would in this respect lose power more rapidly than a copper core, which is not a polar arrangement, and that so a point of comparison might arise and probably also a contrast, even to the extreme case of a copper cylinder with a fine wire axis, which when 4 or 5 inches long should have iron superior in action and when $\frac{1}{2}$ an inch long should have the copper superior perhaps (10578).

10574. A well annealed iron wire about $\frac{1}{20}$ of an inch in diameter was selected, and pieces of different lengths being cut off and fastened on to sticks of wood, so that their inner ends might always be brought up to the same distance (about $\frac{1}{8}$ of an inch) from the end of the residual magnet—these were introduced in turn and the effect on the needle observed. A piece $\frac{5}{8}$ of an inch

in length passing either *in* or *out* was hardly sensible in its effect—the needle end did not move above 0·5 of a degree. A piece being $1\frac{3}{8}$ inch long, made the needle move a degree fully by an *in* or an *out*. A piece $1\frac{6}{8}$ inch long, had more effect and moved the needle $1\frac{1}{2}$ degrees. A piece (the core) $5\frac{1}{2}$ inches long, caused a deflection of about 16°. A piece 10 inches long did not seem to do any thing more (10578). The free space within the Exp. helix, from its entrance up to the end of the Electro magnet (residual), is about 2 inches.

10575. I think that none of the cores, whether iron, copper, silver or whatever they may be, act directly upon the experimental helix through the force of the state (magnetic or of induction) which they have received from the dominant Magnet. The power which is raised up in them by the magnet must, I think, be directed to and engaged with that of the magnet, else action and reaction would not be equal. The power exerted on the Exp. helix is that of the forces emanating from the chief magnet, and which, being disturbed in their direction and relation, do during the act of disturbance affect the experimental helix which is situated within their reach or in their magnetic field. The lines of force which emanate and diverge from the ends of the Magnetic pole, if made to converge, will travel across the turns of the helix and produce a current in it in one direction—or if made to diverge will travel in the contrary course and produce a current in the contrary direction. Iron causes the first effect and sustains it at the stop consequent upon a *to*, and so a *to and stop* produces only one current in the exp. helix and that in a given direction. A copper core in a *to* produces a divergence of these lines of force up to the maximum point of induced current (10557), and a convergence of them from that point to the following stop, and hence gives two currents in the exp. helix in contrary directions for a *to*—the stop producing no effect.

10576. To test this view for iron action, I introduced a thick copper tube into the exp. helix; it was 0·3 of an inch thick in the sides, its internal diameter being 0·1 of an inch and its external diameter 0·7 of an inch—it was 2·2 inches long and was therefore a thick copper lining to the whole of the cavity of the experimental helix. Under these circumstances, the *to* of the *iron wire*

core (10569, 10627), either through the middle of the copper or outside the copper between it and the exp. helix, or up the axis of the exp. helix the copper being taken away, produced exactly the same effect on the needle; the presence or absence of the thick copper tube making not the slightest difference. Now all currents induced directly by the iron core would exist in the copper tube when it was there and not in the helix outside of that tube, whereas on the contrary any action produced by the disturbance of the lines of force of the Magnet would affect the Exp. helix exactly in the same manner whether the copper were there or not (10627), and this shews I think that the action on the helix is not direct from the iron core but from the magnet. An experiment with the Exp. helix and copper lining and a small wire magnet *away* from the great magnet will probably shew this confmd. by action on the helix without the copper tube and little or none with it (10628, 9).

10577. Made some further experiments with the lengths of iron wire (10573), using the E. Magnet urged by 5 pr. Grove's battery. On this occasion of putting on the battery for the first time after four days, had rather a long continuance of the production of currents in the Exp. helix due to the gradual acquisition of the maximum state by the iron (10445, 67, 511), and when that was over and the magnet let down and after 10′ the battery put on again, there was the same effect to a lesser extent. It was only after the battery had been put on to the magnet 4 or 5 times that the latter on after occasions acquired its full maximum state instantly.

10578. With the excited Magnet the iron $\frac{5}{8}$ of an inch long (10573, 4) now sent the needle end 30° or more by a single *in* or *to*. On using a still shorter piece, however, 0·2 of inch long, the effect was again reduced and the deflection was only $2\frac{1}{2}$°. The distance from the iron to the magnet was about 0·2 of an inch, that being the distance of the copper and other cores.

10579. Now proceeded to use the Commutator in Condition i, which should give nearly the best effect for iron (10564), employing the *old iron wire core* of $5\frac{1}{4}$ inches (10449) and the residual Magnet.

10580. I find that this core gives by one *to* or *in* a deflection of 10° instead of $2\frac{1}{2}$ or 3°, as produced by the core used just now

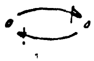
(10569). Found that this was due to its being magnetic, the end nearest the great dominant magnet being in the contrary state to the pole approached. Put a new iron core like that worked with just now (10569) into the wooden case (10526), and this being introduced by hand into the exp. helix, gave about $3\frac{1}{2}^{\circ}$ for a *to* or a *fro*. It is scarcely magnetic, being only so by what it has taken up in this experiment.

10581. This core of *Iron wire*, being attached by it to the machine and sent *in* once, made the needle end move about 2° or a little more; it is less than by hand, because then the whole effect of in is given to the Galvanometer, whereas in the machine a part is cut off by the change of the commutator before the *stop* is arrived at. The importance of the part of the journey so cut off (depending upon its vicinity to the magnet) is in some degree shewn by the difference of 2° and $3\frac{1}{2}$.

10582. When the iron core was withdrawn by the machine, the needle end went about as much as before and in the same direction, because of the change at the commutator. In both these cases the Needle end went East for the standard condition of the connexions and magnet (   ), as on former occasions.

10583. Working the machine regularly, the needle end goes East well and continuously, as on all former occasions, and this is a clear good result with a long core of iron wire, soft and unmagnetic. The commutator is almost the most favourable for iron and, though the Magnet is in the residual state, the power is so great that I may not continue the contact at the Galvanometer except for a moment at a time, and with the Electro magnet excited by battery could not bear it except for the shortest instant of time. This core is about $\frac{1}{16}$th of an inch in thickness. Perhaps making it much thinner and shorter may bring it into comparison with silver in different position of the commutator.

10584. A smaller *core of iron wire* between $\frac{1}{8}$ and $\frac{1}{10}$ of an inch in length and $\frac{1}{32}$ of an inch in diameter, making the same journey and carried up as near to the magnet excited by battery as the former (10581), gave only 8° East deviation by *twelve tos* and *fros* in succession. I must have a still finer wire to compete with copper in this position of the commutator, which is not good for the latter and is for iron.



10585. *As to velocity of motion of the core.* With the unmagnetic iron wire core of $5\frac{1}{4}$ inches long (10580, 10607) and the Dominant magnet in the residual state, *four tos* and *fros* occurring in succession slowly gave 14° East. Then four *tos* and *fros* occurring quickly in like manner gave exactly 14° East. The experiment was repeated several times with the same result (10571, 10607).

10586. Now this ought I think to be the case for iron, which whether at the end of a slow or a quick journey, is in the same place, has risen to the same state, and must have produced in both cases the same amount of convergence on the lines of force (10575, 607). But with *copper* (10657) the effect ought to be very different. If the copper perform its journey in at one time *quickly* and at another *slowly*, it may be a question whether the *same* amount of current has been produced in its mass in the two journeys; the probability is that more current has occurred in the quicker than in the slower journey, because of the lower intensity of the journey and the resistance of the mass to conduction (in a bad conductor, as bismuth, there is no such current produced sensibly). But admitting that the amounts of current are equal, and in that respect the copper to be analogous to the equality in the case of iron (10586), still the two currents will not produce the same result on the experimental helix, whether the action be viewed as a direct one of the copper on the exp. helix, or an indirect one acting on it through its effect on the direction of the lines of force of the magnet. For if the copper advance quickly, the magnetic currents in it will rise to a maximum intensity and then diminish to 0; if it advance slowly, they will continue for a longer time, will advance to a maximum intensity much lower than the former, and then diminish to 0. In both cases the same amount of Electricity will have circulated if it acts as a ring current opposed to the Magnetism of the Dominant pole; and so, causing its lines of force first to diverge and then to collapse, these action[s] will be proportionate to the maximum intensity and not to the whole quantity of the current in the core, and therefore greater with the quick motion than with the slower. Or if we supposed that the current by its rising induced one current directly in the Exp. helix and by its falling the contrary current, still these

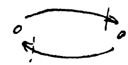
would be proportionate to the maximum intensity and not to the whole quantity of current in the core.

10587. So a great distinction here between such metals as copper and such as iron in their action upon the Exp. helix.

10588. As iron enters the helix to approach the Magnet, its (hypothetical) currents or state rise up to the stop, and do not fall again. So it induces a current in the helix which is not opposed by another due to the letting down. Copper currents rise and also fall by the time it stops, and so this difference.

10589. Copper *in* is in its alternations of state equivalent to iron *in and out*, and hence the difference of character.

10590. Now experimented with the Magnet and battery and this condition of the commutator, using other cores in the machine; so as to verify and clear and establish the correct results of former experiments.



10591. *Copper core*, i. $2\frac{1}{4}$ inches long and pierced (10576). On working the machine moderately, the needle went West most steadily, 25° or more. As before (10427).

10592. *Copper core*, *usual* ( ) i. Well West.

10593. Copper disc core, i ( ). Well West–but not so much as the two former. In the first case, the metal of the core is not dense, being cut up, and in the next the cylinder is not two inches long.

10594. *Copper wire core*, longitudinal, i ( ). If any effect at all, it was a little to the East, as if the effect of the copper were lost and that of a little iron was made manifest.

10595. *Copper filings core*, i ( ). Nothing–or if the least trace, it is East.

10596. *Bismuth core*, i ( ). Nothing–as before.

10597. *Antimony core*, i ( ). Nothing.

10598. *Lead core*, i ( ). Goes West fairly, 7° or 8°.

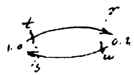
10599. *Sul. Iron small crystals*, i ( ). Slightly East as iron.

10600. *Sul. Iron*, *M.C. axis transverse*, i ( ). Fairly East–quite a clear Magnetic effect.

10601. *Sul. Iron*, *M.C. Axis axial*, i ( ). Well East, 5° or 6°, quite clear.

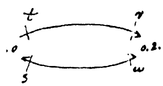
10602. *Red ferro pruss. pot.*, i. ( ). Perhaps a trace East but the effect doubtful.

10603*. I do not know that it is of any consequence, but the polarity of the Electro Magnet and the direction of the coil in the helix are always as represented here.



10604. Now changed the adjustment of the commutator to condition ii, by making the beginning of motion to or fro carry the pin over the division instead of the end of the motion. This is like moving the change back from *r* and *s* to *t* and *u*: for whereas the effect of the advance of the core from 1.*o* to *r* was taken up on one side of the commutator by the then position of the tongues, now it is taken up on the other side, because the commutator change is made at the beginning of the motion instead of at the end.

10605†. In position i, the tongues which carry of[f] the current are on G and E whilst the core is going in and on E and G′ whilst the core is going out. But in condition ii, the tongues are on E and G′ going in and on G and E coming out. The two cases being the reverse of each other for the chief parts of the *ins* and *outs*.

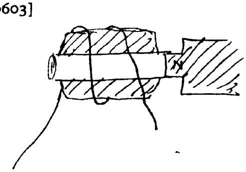


10606. Iron core, ii (   ) and residual magnet. The action is very fair at the needle but now it goes *West*, *not East*. This is of course due to the change in the commutator above described; the chief and predominating and in fact resulting currents are sent in the contrary direction to what they were before, and therefore the needle goes in the contrary direction; for as it [is], the current from 1.*o* to *r* minus the short current from *s* to 1.*o* which forms the current in one case (the lower current has the like form), and the current from *t* to *o*.2 minus the short current from *o*.2 to *u*, which forms the corresponding current in the other case, these are alike in their direction, but as the commutator sends them up to the galvanometer on opposite sides, they must of course give contrary deflexions of the needle.

10607. Under this condition ii, four ins and four outs gave the same impulse at the Galvanometer, 18°, whether the motions were performed quickly or slowly (10571, 10607[1]) confmg. the former result.

[1] ? par. 10585.

* [10603]



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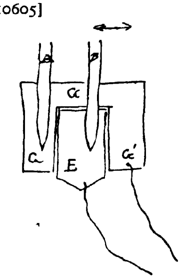
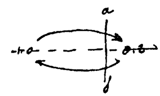
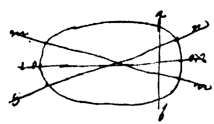


10608. Now employed other cores and of diamagnetic metals in this condition of things.

10609. *Copper core, short*, ii (10576, 10591). Goes well West, 12° or 14°. The needle is not reversed for copper as it is for iron, and this forms a very striking contrast between the copper action of induced currents and such an action as that of Magnetism or of Polar diamagnetism if it existed.

10610. That the needle should not be reversed for copper is evident from the following considerations: first the commutator is reversed for the chief part of the in and out, and therefore, unless the direction of the produced currents had not been reversed also, the effect could not be accounted for; in iron they are not reversed and therefore the needle goes in the contrary direction. To explain this, we must remember that the two currents produced in the exp. helix by iron exist as the core passes from 1.*o* by *a* to *o*.2 and again from *o*.2 by *b* to 1.*o*. But the two current[s] produced in the exp. helix by the copper core (10559) exist as the core passes from *b* by 1.*o* to *a*, and from *a* by *o*.2 to *b*. So in condition i, as the core goes in, it is the current from *s* by 1.*o* to *a* minus the portion from *a* to *r* which goes into G (10605) by the tongue A upon it; and in condition ii it is the current from *m* by 1.*o* to *b*, minus the portion from *n* to *b*, which goes into G′ by the tongue D. Now these two currents are alike in direction and in power and in every other point, and they go into G or G′ (which is the same thing) in the two states of the commutator and therefore must produce the *same effect* at both times, i.e. send the needle end West. Hence the curious difference of inversion by iron and non-inversion by copper.





10611. *Copper core, long*, ii ( ). Goes West well as before, just as in condition i.

10612. *Copper disc core*, ii ( ). West fairly.

10613. *Copper, long wire core*, ii ( ). No trace of action—or if any, very slight West—which may be iron.

10614. *Copper filings*, ii ( ). Nothing.

10615. *Sul. Iron, M.C. axis transverse*, ii ( ). West, 3° or 4°, as iron—curious simulation, so to say, of inductive effect of copper, etc.

10616. *Sul. Iron, M.C. axis axial*, ii ( ). Same as one above.

10617. *Sul. Iron crystals in tube*, ii (   ). West—but less than the above two.
10618. *Red ferro pruss. pot.*, *M. C. axis resultant axial*, ii (   ). Nothing, or else doubtful trace.
10619. *Bismuth*, ii (   ). Nothing.
10620. *Antimony*, ii (   ). Nothing.
10621. *Lead*, ii (   ). West, but very feebly. Battery beginning to go down.
10622. *Copper helix*, *closed*, ii (   ). If anything, the least trace West, but not so much as I expected. Then opened, and now the current in the Exp. helix sent needle end East. This happened a second time, as if there were some real effect. Strange if it should be neither like iron nor copper in this state of the Commutator. Must examine.
10623. In this state of the Commutator, it is evident that we have the power, if at all useful, of *combining* the effect of ordinary magnetic action with the power of induction of currents; and therefore in condition i, we should have the power of combining the power of bismuth, if polarly diamagnetic, with that of the induction of currents. But even then we see no evidence of any effect with Antimony or bismuth. In condition iii, properly place[d], we should separate both Mag. and diamagnetic effects from the effect of induction by currents.
10624. If the *to* of the copper drives or opens out the brush of magnetic lines of force (10575), then a single wire moving across the magnetic curves must do the same; and if so, then the line of force may be conceived of as bending *away from* or *before* the wire—the latter making a continual wave before it as long as it moves. Is it not the exertion of this wave to subside or straiten which produces the Electric current in the moving core or wire?
10625. How will this thought tell on the resistance to the motion of the wire and account for the mechanical force needed? Also, in the case of sawing the space by copper between the magnetic poles, Arago's phenomena, etc.—and how will it bear on a wire carrying a current and so of itself travelling across the lines of magnetic force—and how on the rotation of a magnet carrying the current which makes it move?
10626. If wires could be conceived of acting in succession the

one on another, the first carrying a current say for an instant and the current then stopping, this wire acting on a second should cause in it two successive currents in contrary direction in the time of the first current. This acting on a third outside to it, should produce three currents, and this acting on a fourth, four currents. One may suppose a bundle of wires and the center wire as No. 1, then the others would be the wires lying in the direction of a radius, but each wire would represent the action upon a cylinder of wires equidistant with it from the center. The center may be supposed to carry the electricity of a spark dischge. If the lengths of the following wires* counting from left hand to right represent time, then will not the currents for the time be as indicated by the arrows and their lengths?

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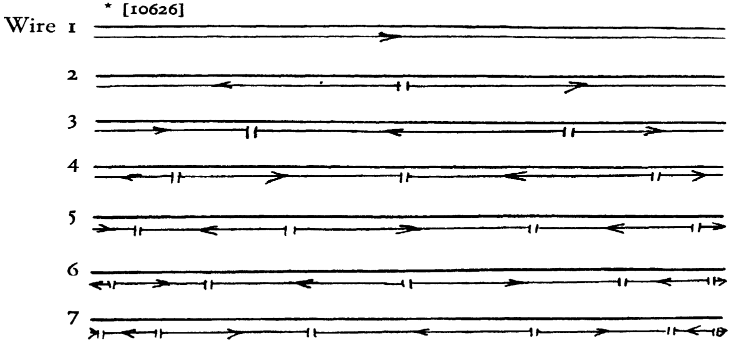
10627. Placed the thick copper tube (10576) in the Exp. helix and used the Residual Magnet. Then selecting a soft unmagnetic iron wire core, I observed its effect *to* and *fro* gave about 3° each in opposite directions; and whether the wire was *inside* the copper, or outside the copper but in the helix, or in the center of the helix (or any other part) and the copper away, the amount of effect was the same (10576, 10682, 8). So copper here does not take of[f] the effect from the Exp. helix.

10628. In place of the unmagnetic wire above (10627) used a magnetised wire (10576, 688, 9). This gave East or West deflexion to the Galvanometer needle according to the pole introduced. When the S pole was introduced (the near pole of the residual magnet being N), it was *East*; when the N pole was introduced, it was *West*. The two were *equal* to each other, but greater than with the soft iron core. The copper tube (10576) being now placed

* [10626]

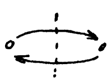


etc. etc. etc.

in the exp. helix and the magnetic iron wire again employed, there was no difference in the amount of effect or kind, whether the magnet wire were in it or not. The effect in fact is always proportionate to the disturbance of the chief magnetic curves of the dominant (residual) magnet.

10629. Then expect that with copper the same thing will happen. But if so, it must happen then through the intervention of the currents which at the same moment are produced in the copper, and so may be referred to their action directly, and indirectly to the magnet through them (10576).

10630. If so, in what manner is this power discharged or carried on, supposing the copper core were slit up and so the currents in it prevented? This is the old recurring question connected with the Electrotonic state.



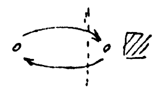
10631. Now arranged the new No. 2 Commutator and think it will answer very well. Adjusted it so as to divide the journey thus, or midway between the two stops, being condition iii (10463, 10500, 12). The residual magnet was used and the *soft unmagnetic iron core* (10627).

10632. Taking only half a journey, so that the Commutator tongues might *not change* sides but give a constant connexion with the galvanometer, a *to* sent the needle end one way and a *fro* the other; and whether these were the halves of the journey farthest from or nearest to the dominant magnet, the *to and fro* of each half equalled each other, and neutralized each other at the galvanometer if taken up in quick succession. If in slow succession, then *following* the swing of the needle they increase its vibrations.

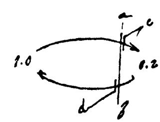
10633. If a whole journey is taken from stop to stop, then two motions appear at the needle. Thus one *in* or *to* sends the needle first East a little and then it is suddenly stopped and sent West by a surplus of force. Of course the whole journey produces only one current in the exp. helix, but this by the commutator is divided into two halves (as to time), of which the second near the dominant magnet is much stronger than the first; and these being opposed by the commutator, gives rise to the effects at the needle and shews why the latter is the stronger. When a whole out was taken, the needle end goes well East, but is suddenly

affected, the force diminishing and only a little East remaining. Here again the first half of the journey out is the strongest and of course it is contrary in its direction to the journey in; hence it gives East deflection strong, and the second or weaker half, being inverted by the commutator, is opposed to and weakens but does not destroy it.

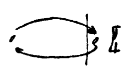
10634. When the machine is worked moderately, it is astonishing to see how well the journey *in* and *out* neutralizes itself—so that the needle remains almost unmoved. When worked quickly for several seconds it crept a little West, but not more than 3° or 4°, which is of course the sum of error for the whole time of working and of very many journeys (10514). Hence this condition iii of commutator is shewn to be that which annuls or sets out the power and effect of iron as was supposed (10562).



10635. Moved the commutator thus, so as to divide the journey into $\frac{4}{5}$ and $\frac{1}{5}$ nearly. This, though approaching the maximum place of action (10562), ought still to give o with iron because the whole of the $\frac{4}{5}$ *in* and *out* ought to be o and the whole of the $\frac{1}{5}$ ought to be o, and it is these which the commutator gathers up. On working the machine moderately, the needle end gradually gathered up to the East.



10636. I traced this effect to the dragging of the commutator tongues on the plate beneath them, which caused that they did not begin to move to and fro the instant the core began to move to and fro; and so lagging behind it, the passage from one plate to the other was not exact as regarded the place of the core. Thus in the *to*, the end of the core was more advanced in its journey to stop 2, when the tongues passed the line *a b*, than it should be; and in the *fro*, was more advanced from stop 2 when the tongues repassed the same line, so that it was as if the stops had been made at *c* and *d*, which is a form of commutator condition i, and should therefore give east deflection. I found that they did so drag, the tongues continuing longer on the *o*.2 side from *o*.2 to *b* or *d* than from *a or c* to *o*.2, and when I made them drag a little more, the East resultant was increased. This is probably the cause of several anomalies in former cases (10649, 75).

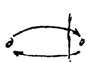


10637. Adjusted the commutator so as to divide farther on, the portions being about as $\frac{6}{7}$ and $\frac{1}{7}$. Same result as before (10635),

but the East gathering was quicker and stronger, because the commutator changes and therefore its shiftings tell in a more active part of the journey (near the dominant magnet).

10638. Arranged the Commutator so that the tongue barely passed to the TO *stop* side. As before, the Needle *waggles* strongly and gathers *East*. Altering the commutator so as to* on the other side or near the *fro stop*—the effect was the same in kind but the East gathering was very much slower. The amount of drag at the tongues is the same in both cases, but it occurs in a place of strong action at first and of weak action at last, and hence the difference.

10639. Now that there has been a little wear, I can even *see* that the tongue tips hold on the brass and do not change their place through a certain small extent of motion right and left of the lever and the core and even of the roots of the tongues or places where they are fixed to the carrying piece. I have for the sake of secure contact made them press too hard on the plate beneath, which is already beginning to wear in their track. I must lessen this pressure and perhaps stiffen the tongues by a frame connecting them (10514).



10640. *Copper core*, iii (10502). Tried one experiment with this, the most favourable condition of commutator (if at the right place) for the inductive effects of copper, making the commutator divide about $\frac{5}{6}$ and $\frac{1}{6}$. I used only the residual magnet. Still I was able to obtain a West deflection, amounting to about 1°. It was quite sure and distinct.

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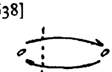
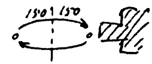
10641. I have stiffened the tongues of the commutator against the effects of lateral force and diminished their pressure upon the under plate, so that they move easily, well and with continual contact. The Magnet in the residual state is now in use (   ) and its end is about $\frac{1}{5}$ of an inch from the end of the exp. core when at the nearest (which is nearer than yesterday). The commutator is to have continually the iii condition (10463, 631), but the place of transfer to be shifted (simultaneously on both sides of the journey) to different distances from the stops 1 and 2. The whole distance of the journey is represented on the plate where the

* [10638]



tongues travel by 300 parts of a divided scale, which I will use for the present, and the different distances from the place of change to the stops I will represent by the parts of this scale, as thus.



10642. The first adjustment is such as just to let the tongues pass over before the stop after *to* or *in*, i.e. *o.2*, and the degrees are about 290 and 10. An *iron wire core* (10569, 80), nonmagnetic, was in the machine. When carried *in* by the machine, the needle end went 8° East—and when carried out, 8° West.

10643. Beginning with the core *out* of exp. helix, the machine was worked steadily for some time: the needle end first gathered up to the East gradually 5°, then stopped—then gradually returned and gathered up to the West 10°, when the working was suspended.

10644. Again—beginning with core *out* and working moderately, the needle end went 4° East very gradually—then stopped, returned and went $1\frac{1}{2}$° West; then returned from that and stood at 0°, the machine working all the time.

10645. Again—beginning with the iron core *in*, after it had been in the Exp. helix about two minutes, and working moderately, the Galvr. needle went West 4°—then returned—passed to the East a degree or so and then returned to 0°, the machine still working. The same result was given in a second experiment of the same kind.

10646. A single in gives East effect: and a single out gives West effect.

10647. I had both these effects yesterday, and found then as now that the effect was soon over and the needle after that stood at 0°. I considered it as probably due to this, that when the iron wire core was *out*, its in and approximation to the magnet (dominant) gave it a certain degree of magnetism, which it could as a *mean* retain during its successive journeys to and fro during the rest of the experiment, and that this assumption of a magnetic state was that which gave the effect of swing to the East, as it would do: having acquired that state it would retain it, and therefore no further effect due to gain or loss of power would occur, and the effect would sink away in vibrations to and fro and the needle be left at 0 by the equality of the other actions.

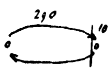
10648. On the contrary, when the core was first *in* and then the experiment began, its first state of magnetism would be higher

than the *mean* state for the rest of the experiment, and the fall to this mean state would produce the first impulse to the West of the needle, which would gradually pass away by a vibration or two and leave the needle at 0, under the equal to and fro actions of the core once in its constant state. The experiments of to-day confirm me in this view.

10649. So now there is but little dragging of the tongues (10636) and the iron action is rendered nul as a whole or neutralized in condition iii of the commutator (10564) even for this extreme case.

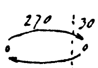
10650. An iron core of five thick wires, equal to ten times the mass of the former core (10642) and of the same length, was used. It gave exactly the same East and then West swing, settling at 0°, and continued at 0° during a considerable time of machine action.

10651. Now experimented with copper and other cores for which this condition iii of the commutator is (at least in one position which has to be found) the best (10559).

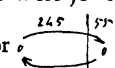


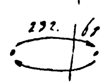
10652. Short copper core (10627) and residual magnet. The needle end went $\frac{1}{2}$ a degree to the West. The arrangement therefore is favourable in principle, for even the residual magnet brings out the effect.

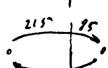
10653. Put the V. Battery of 5 pr. Grove's plates on to the Magnet and had the first effect (10445, 67, 511, 77) for a considerable time. After this was nearly over, worked the machine, and then the Galvr. needle end easily and readily went 40° West.

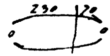


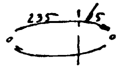
10654. Continued to use the V. Battery to the magnet and made the commutator thus. There was a better effect than before, for the needle end went West 50° easily.

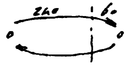
10655. Commutator  Needle end West 50°. About as good, or nearly as good.

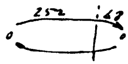
 Needle *West* 55°.

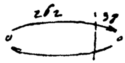
 West to 45°—not so good as the last certainly; so proceeded to diminish the right hand side.

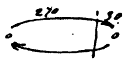
Commutator  West nearly 50°.

 West 50°.

 West 45°.

 West 51°. A good position.

 West 52°. Good.

 West 44°.

10656. I should think that 260–40 would be a good division for the present condition. The numbers would probably be affected by changing the distance of $\frac{1}{5}$ inch (10641) between the magnet and the iron core. The battery has of course been falling in power during these experiments.

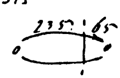
10657. In reference to the important effect of velocity in the case of copper, etc. (10554, 10571, 10586, 607), I made certain experiments with this small copper core and the commutator constantly at* With a certain moderate velocity of working, as that employed above, the needle went West 50°, but with quicker working it swung up to 72°.

10658. Again, ten revolutions of the great wheel or 40 *tos and fros* at a quick rate sent the needle above 80° West. Other ten at a more moderate rate sent the needle only 39° or 40° West, and other *ten* made still more slowly sent the needle only 20° West. Thus the influence of velocity of journey is very great.

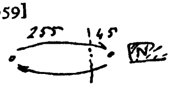
10659. Then adjusted the commutator to†, as being a very effectual position, and retained it there for all the following experiments, the velocity being nearly the same average moderate velocity in all (10657, 8).

10660. *Copper long core* ( ). West 48°.
Copper disc core ( ). West 25°.

* [10657]



† [10659]



Copper long wire core ( ). Nothing.
Copper filings core ( ). Nothing.

10661. Copper wire helix ( ) closed. West, 5° or 6°.
" " " open. Nothing.

10662. Lead core first ( ). West 4° or 5°.
Lead, chinese, smaller diameter core ( ). West $3\frac{1}{2}°$.
Lead spiral plate ( ). Nothing.

10663. Tin, solid core ( ). West 7°.
Tin disc core ( ). West 6°.

10664. Silver discs ( ). West 45°.
Silver–divided–precipitated by copper and packed hard into a glass tube. No trace of action.

10665. Platina core ( ). Clearly West about 3° or 4°. It is just possible, as Platina is a magnetic metal, that here there has been a separation of the magnetic and the induced effect.

10666. Gutta Percha core ( ). No trace of action.

10667. Sulphate of Iron, M.C. axis transverse ( ). Nothing.
" " M.C. axis axial ( ). Nothing.

10668. Red ferro prussiate potassa ( ). Nothing.

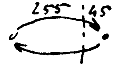
10669. Short copper core ( ). West 38°. Shewing that in all these experiments the arrangement had been good and the connexions perfect.

10670. Bismuth core ( ). Apparently West action, about 1°.

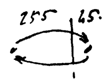
10671. Antimony, solid core ( ). More distinct West action, about 2°. Antimony in powder ( ). Nothing.

10672. I have a doubt of these action[s] of 1° or 2°, which I must remove by experiment hereafter (10678, 83, 4). When after making the contact of the V.B. at the magnet one has waited a moment and then completes contact at the Galvanometer, there is often a trace of motion of the needle, which then passes away; but after the above experiments were made, I thought I observed also a peculiar effect with antimony, as if the Antimony core, without working, sent the needle right or left according as it was in or out of the Exp. helix–but as the commutator changed in the two cases and as there was a residue of effect from the Dominant Magnet, it may have been the complication of these effects, all of which were small.

10673. *Copper and Iron combined* in this condition of the Commutator. The short copper cylinder had two inches of soft unmagnetic iron wire about the $\frac{1}{16}$ of an inch in diameter fixed in its center, so as to form axis to it. This arrangement on the machine, when using the excited E. Magnet, sent the Needle well East because of the power of the magnet and the dragging of the tongues (10636, 49, 75) and whole apparatus. It shews however that the effect of the iron is to go East, and is therefore not merely neutral but contrary to that of copper, silver, etc.



10674. A length of $\frac{1}{8}$ of an inch of this same wire, fixed in the end of the core nearest the Dominant magnet, could not however overcome the power of the copper, for the needle then went West 30° or more. Yet this core was very magnetic when the end was brought near a magnetic needle, and the piece of iron without the copper at the end of a wooden core made the Galvanometer needle creep East, even in this condition of the commutator.

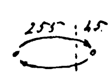


10674½. Another iron wire, 2 inches long but smaller in diameter, being perhaps $\frac{1}{50}$ of an inch in diameter, was placed in the copper core (10673). This compound core sent the Needle East also slowly by creeps.

10675. This creep depends not merely upon the drag of the tongues but upon other parts of the machine, especially at the axes and pins here and there which are now somewhat worn; hence on moving the exp. core and through it the commutator, it is easy to see that the former can travel a little way backwards and forward—before the tongues started. I blocked up one of these axes with wood, so as to remove a certain amount of shake, and then the creep was much less (10649, 10636, 73).

10676. Still, we have, to a certain degree, the power of separating the effects of copper and iron, and where not of separating, we have of opposing them (10674).

1 DECR. 1849.

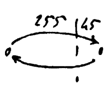


10677. The apparatus in order—the commutator iii. The voltaic battery 5 pr. Grove's—freshly charged. No exp. core as yet. On connecting the Battery with the Magnet, the intensity of the action of the Magnet rose, producing East deflecting current in

the Experimental helix a full minute and a quarter after the moment of contact. The connexion being broken and a couple of minutes allowed to elapse, the battery was again connected and the same rising effect was observed, but continuing probably not more than $\frac{1}{4}$ of a minute. In both these cases, after the needle had returned to zero, it still continued to be affected irregularly, passing a few degrees on this side or on that side of zero with an unsteady motion and changing direction quickly—as if there were quite a magnetic storm in the Electro magnet. After several experiments with the magnet and some weakening of the power of the V. Battery, this was still observed to be the case. Then, however, the disturbances right and left were small, perhaps $\frac{1}{2}$ a degree or sometimes a degree. I think they are due to variations in the force of the battery current, which telling on the magnet, by it tells upon the Galvanometer. The effect would probably give a good mode of studying these slight differences of current.

10678. As it is, though it does not interfere with experimental results with diamagnetic giving deflection from 5° and upwards, still in respect of such bodies as Antimony and bismuth, where the effect is very small, if any, great care is required to avoid confounding these irregularities with results supposed to be produced by the exp. cores (10672).

10679. I shifted the helix on the core (10379) so as to bring it close up to the shoulder in the latter, and then removed it away even to a greater distance than in the ordinary experiments. The disturbances appeared at the galvanometer and apparently the same in either case. I do not think that any current is communicated directly to the galvanometer.

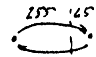


10680. Placed the *small copper core* (   ) in the machine to test the action. The Needle end went well West with moderate working. After a while, repeated the experiment. Needle end went West 60° with moderate velocity.

10681. *Narrow lead core* (   ). Went West but only feebly. No doubt all my results would be far better with cores of larger diameter and shorter length, and perhaps with a much larger experimental helix.

10682. *Silver*. Have made a core of 36 silver fourpennies for a narrow core. The needle goes *West* 60° with moderate average

motion. With a slower motion, the needle end went 32° West. Then placed a copper lining in the exp. helix, being a tube $2\frac{1}{2}$ inches long, 1 inch external diameter and $\frac{6}{8}$ internal diameter (so $\frac{1}{8}$ thick), and repeated the experiment with the same velocity. The small silver core produced again effect *West* to 32°. So the copper does not interfere here, any more than it does with iron (10627, 8).



10683. *Bismuth core* (   ) iii—no trace of effect.

10684. Antimony core (   )—a trace West.

10685. Solid copper wire helix. I have made a helix in the form of a cylinder,   inches long and   of an inch in diameter, of   feet of uncovered copper wire $\frac{1}{16}$ of an inch in diameter, hoping that the film of oxide on its surface would be enough to prevent contact. When used as core, it gave West declination to 45°, but this was the same whether the ends were open or closed; in fact the lateral contact of the spirals was as good as contact at the extreme ends. So it tells nothing.

10686. *Used the old helix*, which is very poor (10388, 9, 90) and soldered the ends together. In one experiment I obtained West 8° and in another West 10°. Then unsoldering the ends, I could obtain no effect at all. A very good experiment, as shewing the *undisturbance* of the Magnetic curves when no current can take place in the core (10390).

10687. The short copper core in place (10627, 52) and the Exp. helix close up to the Magnet shoulder—West 60° was obtained. The exp. helix was now withdrawn $\frac{1}{4}$ of an inch from the shoulder (10379, 679) and the experiment repeated; still 60° *West* were obtained.

10688. The exp. helix being removed from the Electro or residual magnet but still connected with the Galvanometer by simple connexion, was used with the copper lining (10576) and a core of unmagnetic iron wire. When the core was put in or taken out, there was the least possible effect at the Galvanometer, but it was the same whether the copper lining was there or not (10627).

10689. Then a steel wire magnet was used in place of the soft iron (10628). Entering, it deflected the needle 7°, and on being withdrawn, 7° in the contrary direction were traversed; but the presence or the absence of the copper lining made not the slightest difference.

10690. It is curious to consider, what is the difference in action when the copper is there whole, when there are currents; or *slit open*, when there are *none*? The final result is precisely the same. Supposing also the space between the exp. helix and the magnet to be filled alternately with copper and air, with non-conducting and conducting substance, what is the difference of action in this space to allow of the same final result?

17 DECR. 1849.

10691. Repeated the expt. of Reich described by Weber (10050) and can in no way obtain the results he mentions. Bismuth and Iron had the same non action or action to me as before. Further, whatever the action of iron or ferruginous bodies, they were not so influential upon the little needle when it was between the chops of the horseshoe magnet as when they were approached directly to the same little needle, and far less than when approached to an astatic needle (10050).

18 DECR. 1849.

10692. Employed the Great Electromagnet horseshoe with 10 pr. of Grove's plates to test Magnetic condition of bodies tried in the former machine expts.

10693. *Platina core* ( ). Magnetic, vibrating between the poles in one direction or *to*, in less than a second.

Sul. Iron crystals in tube ( ). Magnetic and pointing.

Gold—half or whole sovereigns ( ). A little magnetic.

Shilling—clean washed ( ). Magnetic—went up to the pole.

Tin core, common ( ). Diamagnetic with this power.

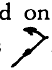
Tin core, discs ( ). Do.

Chinese tin core, short ( ). Diamagnetic.

Chinese lead core, narrow, long ( ). Diamagnetic.

Do., thicker, shorter ( ). Diamagnetic.

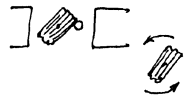
10694. Now as to revulsion of masses made up of discs or plates.

Gold. Seven half sovereigns in a bundle, when standing thus*, went up, i.e. from  to  very well upon making battery magnet contact, and on breaking contact were well revulsed, going of course thus . The bundle was magnetic from the magnetism

* [10694]

of the coins and tended to stand thus, as was natural, the length of the pieces of coin being then axial. 

10695. A piece of pure *gold* foil, folded up into a square bundle, though very small, shewed the advance and revulsion very well. 

10696. *Silver*—a bundle of 10 fourpennies—is magnetic and points thus.  The magnetism interferes with the revulsion effect, for the coming up cannot be distinguish[ed] except by a little added velocity from the magnetic coming up, but if whilst the magnetic force is on, the bundle be blocked thus, then on taking off the magnetic force the revulsion is evident and sends the bundle thus. 

10697. Another bundle contains 17 fourpennies and the length of the cylinder is more than the diameter. This also is magnetic and still the discs stood axial thus.  When this is blocked as before (10696), on causing the magnetic force to cease, the revulsion occurs strongly, i.e. the axis of the cylinder tends to place itself axially; the effect does not look like revulsion because the greatest length of the cylinder goes axially, but that is because of the division into discs and shews the power of that division.

10698. *Platina foil bundle*—very magnetic and no revulsive effects. The magnetic force much and the conducting power (favouring revulsion) little, and so no signs of the latter. 

10699. *Tin foil bundle.* Not magnetic—will stand in any position. Shews the advance and the revulsion very well indeed—here division very advantageous. 

10700. *Zinc foil* bundle. Is Magnetic, but when blocked in position (   ), shews the final revulsion. 

10701. *Lead leaf bundle*—very magnetic—when blocked shews small effect of revulsion, though lead a bad conductor—division into discs helps here sensibly. 

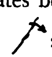
10702. *Copper foil bundle.* Not magnetic—will stand any where in Magnetic field—advances when contact made and very strongly revulsed as magnet falls.  But there is a particular effect here which none of the other metals shewed me except Tin slightly. When the copper is revulsed, it passes back,  and swinging beyond the equatorial position goes on towards the next axial position; and if it were a solid block, would from former experiment make 3 or 4 revolutions before it stopped by torsion of the suspending thread; but now it does not arrive at the second axial position,

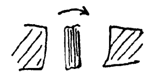
but is repelled back from that, taking the following position[s] in succession.

10703. This of course ought to be the case, provided the currents produced in the revulsed metal last longer than the time required by the copper to attain to the equatorial position or a little beyond it; for supposing that the fall of the Electro magnet force and the fall of the currents produced by it in the copper could be suspended, for a while of course, the copper bundle would vibrate about the equatorial position, i.e. the axis of the electrical currents in it would, would vibrate about and at last rest in the axis of the Magnetic field. Now in copper the currents produced are so strong as to make the time of passing beyond the equatorial position very short, and so easily produced that they are present for a longer time than in bad conductors, and so limited in their course by the disc division, that this vibration actually occurs and is a beautiful extension of the revulsion effect. The tin bundle approached to this effect thus—that it stopped after passing the first equatorial position, and that by an effect which, if it had been stronger, would evidently have made it return. The copper bundle was about $\frac{1}{2}$ inch square and $\frac{1}{4}$ of an inch [thick] and in that thickness included 72 folds of copper foil, slightly oxidized on the surface. The tin foil bundle was 0·6 inch square, 0·2 thick and also 72 folds of foil.

10704. *Bismuth* (10416). Was melted in an iron ladle, portions poured out on to dried foolscap paper placed on a hot copper plate and immediately flattened out by a smooth, flat, hot copper block. In this way, plates of it could be obtained easily less than the $\frac{1}{50}$ of an inch thick. Those procured at first were diamagnetic and gave no magnetic trace of iron. The latter portions of the bismuth require a *higher heat* to melt them, and they appear to be magnetic and contain portions of iron.

10705. The first plates were broken up into discs about 0·8 of an inch in diameter, and 12 of these made into a bundle 0·2 of an inch thick. This bundle was diamagnetic and so strongly so between pointed poles that it was difficult to observe any revulsive effect. But with flat faced poles the effect was evident; for if the battery contact was made, the plates being thus, and still, there

was immediately a tendency thus , which immediately after



gave way to the diamagnetic effect that set it back into the equatorial position. Further, if the plates were swinging thus, and then contact made, the momentum and the advance effect would tend to take them forward thus , to a position at which they would stop and from which they would return diamagnetically; but if the magnet battery contact were broken before this return commenced, then the revulsion was seen giving them a kind of blow impulse backward, different to the diamagnetic oscillating impulse which continuance of battery contact produces.

10706. Another bundle of larger plates of bismuth gave the same result.

10707. I do not know whether there is any general Magnecrystallic power in the bismuth plates, due to the manner in which they are cooled. A plate of 0·8 of an inch diameter, set equatorially between flat faced poles 1·8 inches square and about an inch apart. Probably these sized poles do not give an uniform field of magnetic action at that distance of one inch.

10708. *Antimony*. Plates of antimony were prepared in the same way, dismissing the paper (10714[1]), and using much hotter copper. A bundle containing 10 antimony plates formed a rough cylinder 0·7 of an inch in length and about 1·1 inches in diameter. This bundle was a little magnetic. But it shews the revulsing power very fairly.

10709. A single round antimony plate 1·2 of an inch in diameter and about $\frac{1}{20}$ of an inch thick—was not magnetic but diamagnetic a little. It shewed the advance and revulsion well—far better than the bundle (10718[2]). It would almost stand in any position in the magnetic field, though it tended to go equatorially. Another clean similar plate produced the same effects.

10710. Repeated the expt. of   of placing a bar of iron in the Magnetic field under the equatorial position which a bar of bismuth suspended in the field would take. There is no doubt that it makes either a bar of bismuth or antimony or phosphorous point better and quicker, but this appears to me to be only because it makes the lines of Magnetic force decrease more rapidly in force

[1] Par. 10704. Pars. 10702–10711 inclusive were wrongly numbered 10712–10721 in the MS. and afterwards altered.

[2] Par. 10708.

than before, which is easily seen if the ends of the bismuth bar in its equatorial and axial position be considered. I do not see any *additional* evidence of polarity there.

10711. When a cube of bismuth was suspended in the magnetic field in different parts, it also was affected in various ways by iron in the field, but I saw nothing beyond the law of going from stronger to weaker places.

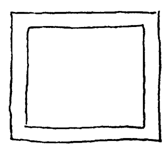
2 JANY. 1850.

10712. *Pure Platinum.* Have prepared pure platinum by taking a solution of clippings—good clean clippings—precipitating it by Mur. Ammonia—washing the precipitate well in acid water—reducing it by heat—redissolving it in pure N.M.Acid—reprecipitating by Mur. ammonia—washing the precipitate as before and reducing it by heat, very carefully in German Glass tubes. I find that by pressing this spongy platina together into a cake as close as possible between platina foil—wrapping the bundle up in clean foolscap paper and hammering it carefully and well on a small anvil by a smooth hammer, I can get a very good plate of platina coherent and dense and fit, I think, for magnetical experiments. Both the plate and the Spongy platina are put carefully aside for future expts. on the possibility of shewing both magnetic and diamagnetic conditions in a pure body: which I doubt.

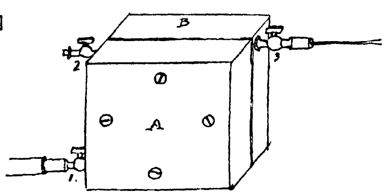
10713. *Pure Palladium.* Some very good Ammonio muriate of palladium was digested in hot dilute Nitro muriatic acid for some days—then well washed—reduced by heat in German glass tubes—redissolved in pure N.M.Acid—the solution evaporated to dryness and redissolved with a little pure M. Acid—and then precipitated by a solution of pure cyanide of mercury. The precipitate was well washed—dried slowly—and reduced in a well cleaned platinum crucible over a gas lamp and air flame urged by a blow pipe. Every care was taken to exclude impurity or iron. It does not appear to be malleable so as to adhere under the hammer, but probably the pieces will serve for expt. as they are. Set aside carefully, for future expt.

7 JANY. 1850.

10714*. *Diamagnetic or magnetic condition of gases.* Iron chamber. Two blocks of soft iron, each 1 inch thick and 3 inches square, with filed and flattened surfaces, and a square frame of copper cut out of a sheet of copper so as to be 3 inches in the side and 0·3 of an inch deep all round, were put together and held firmly by four copper screws, so as to form a very strong iron box having an interior 2·4 inches square and only $\frac{1}{60}$ of an inch in



* [10714]



depth, for that was the thickness of the copper frame. After being put together, the edges at the junction were touched all round with white hard varnish and also the countersunk heads of the screws, so as to make the box air tight. The face A and the corresponding opposite face were quite flat. Three stopcocks were fixed into air ways made in the body of the iron. Stop cocks 1 and 2 were fixed into the iron piece A, so that the inner terminations entered just in the corners of the chamber above and below. No. 3 was fixed in piece B and had its entrance up in that corner. Any gas therefore sent in at 1 and out at 3 would sweep right across the chamber. Cock 1 was connected by a piece of vulcanized rubber tube with a drying tube filled with chloride of calcium, and that with a floated Gas jar containing any gas that was to be sent into the chamber. It was easy then, by opening the cocks 1, 2 and 3, to fill the chamber with any gas and sweep out any gas previously occupying it.

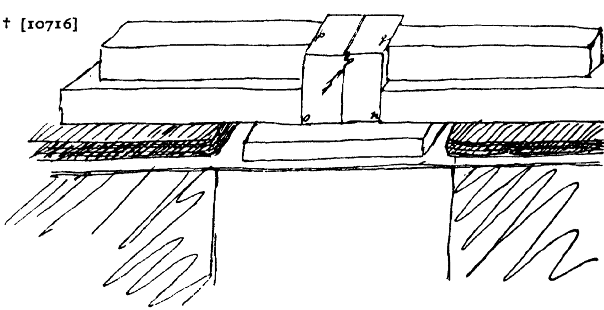
10715. Indicating guages or tubes were formed by drawing out fine glass tubes and fitting one by a cork and cup and cement on to cock No. 3; the form of the guage was this*, but they were about $\frac{1}{3}$ of the size of that drawn[1]. Having this form, a little drop of spirit coloured by cochineal being put in at *m*, it advanced to the narrowest part of the tube and form[ed] a cylinder which tended to remain there, and which from the fineness of its diameter would be a very sensible indication of any increase or diminution of the bulk of the gas in the box A B when cocks 1 and 2 were shut and 3 open (10760).

10716†. This box was placed in the Magnetic field between the poles of the great Electro magnet, and the iron terminal pieces

[1] The diagram is reduced to $\frac{3}{4}$ scale.

* [10715]

† [10716]



on the tops of the pole faces brought close up against the face A and the corresponding face of the box, so that it formed as it were part of a built up keeper. In fact, it is evident that the magnet poles were continued on until they were only $\frac{1}{60}$ of an inch apart, being indeed the inner opposed faces of the iron box. Now it was thought that if there was any expansion of a diamagnetic gas, or any condensation of a magnetic gas, in the Magnetic field, it would be sensible in this box, where the pole faces formed the interior of the box were only $\frac{1}{60}$ of an inch apart, where the field therefore would have great intensity of power and where the gas would be in close proximity to the poles.

10717. First *Air* was employed in the box, the cocks 1 and 2 were opened and also cock 3. A minute cylinder of the indicating fluid was introduced into the guage, then cocks 1 and 2 shut and all was found tight and in order. Care was taken not to touch the iron box with the hands. The Electro magnet was urged by 10 pair of Grove's plates. Now when contact was made, the indicating fluid moved immediately about $\frac{1}{10}$ of an inch, and outwards, as if the air within the box had been expanded, and when the battery was unconnected the fluid immediately returned. The motion did not come on suddenly, but gradually, and go off in the same manner; the expansion continued to go on perhaps for a second or a little more, and took about as long to return.

10718. Now this effect cannot be a permanent effect of temperature, for it returns at once. It may be either a diamagnetic effect or an effect of the great compression of the box, though that is very strong, either upon the iron itself or upon the copper frame between. When I put strips of wood between the terminal pieces and the box, as at *n*, *o*, *p*, *q*, to take off part of the pressure, the effect was much reduced; but then the power also within the box must have been diminished by this separation of the irons (10734, 75).

10719. Filled the box with *Oxygen*, which is either a magnetic or the least diamagnetic of the gases—and repeated the experiments with all care. There was expansion or a corresponding effect in the guage and just to the same amount as before (10723, 34, 73, 6).

10720. *Nitrogen*. Precisely the same effect as with air and oxygen (10734, 69).

10721. *Carbonic acid gas.* As with Air, Oxygen and Nitrogen (10734, 74).

10722. *Nitrous oxide.* As with Air, oxygen, nitrogen, and C.A. Gas (10734, 72).

10723. *Oxygen* again. As before (10719, 34, 73, 6).

10724. *Hydrogen.* There was a continual absorption of the gas, both in the chamber, perhaps by the white hard varnish, or the film of oxide of iron, and also in the guage cap, perhaps by the cork and cement holding the guage. It was to such an extent that the guage liquid was never quiet and I could make no observation as to the magnetic force.

10725. *Coal gas.* Presented the same signs of absorption as the hydrogen, but the effects were not so quick, and after the absorption had gone a little while and become slower, then as the liquid was moving in slowly, throwing on the magnetic power would stop it, and on taking off the magnetic power, it would retreat inwards more rapidly; shewing the same effect as with the other gases, but mingled with the effect of absorption.

10726. Now took the chamber down, with its guage, placed it on a table with face A downwds., put a board on the upper face and applied the pressure of 120 or 130 lb. suddenly on it; there was advance of the fluid, shewing that this pressure had been able to diminish the capacity of the chamber. And as the different gases give like effects in kind and degree, there is no doubt, I think, that in their case it is the effect of the pressure brought to bear upon the box by the powerful magnet.

10727. Another very beautiful effect, indicating both the sensibility of the indication and the progress of a wave of heat through iron, also occurred. As the box laid sideways, the hand was brought against the top surface for a moment only and then removed; there was no immediate indication on the guage, but in about a second or a little more, the guage moved outwards, shewing the expansion of the air by the heat of the hand which had then travelled to it. I do not think more than a degree of heat could have been communicated to the superficial portion of the box, and the iron was an inch in thickness, yet thus beautifully it passed through.

10728. So gases as various as Oxygen, Nitrogen, Carbonic acid,

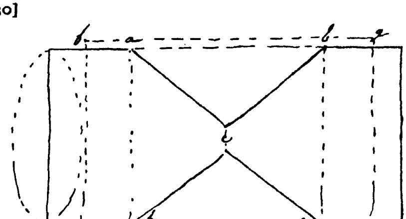
and nitrous oxide shew no difference amongst themselves or from air—though they are so very different when tried as streams one in another. Hence this method seems defective in principle, or at all events in sensitiveness; and yet as seen by the heat effect—it is very sensitive. Certainly there was no hopes for any optical results (10277, etc.) since there are none here. I think Plucker must have been mistaken in his result, and that my old observation was right.

10729. Leaving the iron box containing air in its position between the poles (10716), I made and interrupted battery contact perhaps 30 or 40 times, but I saw no signs of heat having been acquired by the iron box. Cards were interposed in this case between the box and the adjacent pieces of iron on the magnet poles.

21 JANY. 1850.

10730*. Thinking that the former iron chamber might be ineffectual because of the position of the parts, especially if there should be any reason to suppose a difference in the direction of any force exerted, and which might in the former case affect the result, in that as the particles of a diamagnetic body retreated they would tend to move from the iron faces parallel to the lines of magnetic force; thinking thus, I had another arrangement made in which the retreating diamagnetic would cross the lines of magnetic force. A cylinder of soft iron, 3 inches long and 1·7 in diameter, had a portion turned away in the middle so as to give it an hour glass form; from *a* to *b* was 1·6 inches, and the thickness at *c* 0·32 of an inch. Then a copper tube, $2\frac{1}{2}$ inches long, 0·1 of an inch in thickness, and just large enough to go over the iron, as at *d*, *e*, *f* and *g*, was made, being nicely turned inside and outside, and this with a little soft cement was attached to the iron so as to inclose the air space *a*, *c*, *b*, *h*, *i*, *c*, which formed a sort of ring chamber within, in which the gases experimented on were to be confined. Three stop cock apertures were in this copper tube, the one at the top and one on each side. Into these were fitted the three former stop cocks (10714), and one of them on the side was

* [10730]



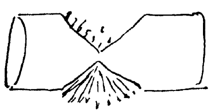
connected as before by a drying tube with a jar of gas, the other horizontal cock had the guage attached to it and the third or upper cock was to equalize the pressure with that of the atmosphere and give passage outside for any gas contained within.

10731. The guage was made with more care than before, and widening from the middle either way, so that the drop of fluid always stood in the narrow part and was not likely to run to the inner or the outer end, except under the exertion of force (10715).

10732*. When the chamber was put in the place of the former chamber, it was found that the liquid in the guage was continually moving because of the changing temperature of the copper, etc. box—a mere breathing near it made the fluid move. So made a jacket of triple flannel to go over it and the poles as far as *a*, *b*, letting the stop cocks come through holes cut in the jacket; by this precaution, and also by using a wooden key to turn the handles of the Stopcocks, I had a steady guage and could experiment.

10733. First ascertained that all was tight by a little pressure on from the air jar and the opening and shutting of the cocks, etc.; [illegible] all tight, yet delicate; and then proceeded to experiment, using for the magnet a battery of 10 pr. Grove's Plates.

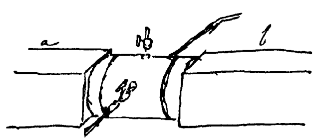
10734. *Oxygen.* On putting the battery on or off, there was no trace of motion in the indicating fluid; no signs equivalent either to expansion or condensation. The same was the case with Nitrogen, Air, Nitrous oxide and Carbonic acid. There was no trace of any difference or of any action (10719, etc., 66, etc.).



10735. Having satisfied myself in this respect, I took down the apparatus, removed the copper outside, replaced the iron alone between the poles and proceeded to examine its action on a flame or on smoke. When the magnet was excited, a taper flame was well affected in the angle, going outwards and shewing the expected result.

10736. When green taper smoke was applied, it went in all directions from the angles, i.e. if at 1 it went towards 2 or 3, or if at 3 it went towds. 5, if at 5 towds. 6. The tendency outwards was not merely at right angles to the lines of force but in all directions from stronger to weaker places of action (10779).

* [10732]

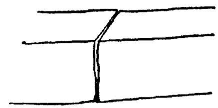


10737. When the old poles ( ) were experimented with, the same result occurred, and that when the position was changed by turning them round 90° or more on the magnetic axis. The smoke always tended to proceed from strong to weak places of magnetic action, even along the surface of the iron.

10738. Placed the poles as above (10737) and a plate of thin mica over them, and a sol. of proto sulphate of iron on it. When the magnet was active, the fluid drew together and was heaped up in the places of strongest action, and there was no signs of any current—as if it tended to go in a particular direction and so circulating; but it tended to accumulate in a static condition due to its magnetic character.

10739. When water as a diamagnetic body was used, it tended to go from strong to weak places of action in *all* direction[s] and not in a particular one either parallel or perpendicular to the lines of magnetic force (10787, etc.).

FOLIO VOLUME VI
OF MANUSCRIPT



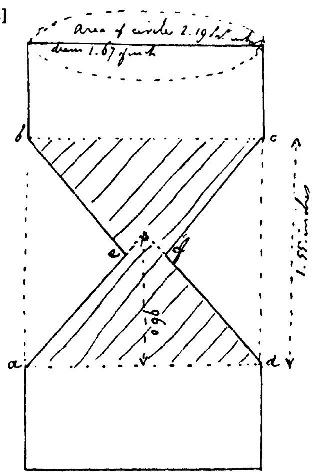
10740. When the plane ends of the poles were separated only by[1] 2 thickness of card, the ends being 1·5 inches square, and the power of the battery was on, Saturated sol. proto sulphate of Iron could not be kept in the little space between the two poles up to the top, but only a certain height above the interposed card. When the power was off, a part of the fluid ran out. The difference in level was not so great as I expected, being, in the middle of the two faces, about $\frac{1}{5}$ of an inch. This counteraction of gravity was of course the measure of the magnetic attraction and was less than I supposed would be the case.

10741. Still, if there be an expansion or contraction of diamagnetic or magnetic gases between the poles, the effect must be very small indeed, or it would have been sensible in one or the other of my apparatus.

10742*. As the part of the cylinder concerned in forming the chamber is 1·67 of inches in diameter and 1·55 inches in height, so its whole capacity *a b c d* is 3·3945 cubic inches. As the cone *b e d f c* is 0·96 of an inch high, so its capacity will be 0·7 c.i., or one third of the capacity of a cylinder 1·67 in diameter and 0·96 of an inch high. Hence double that capacity, or 1·4 c.i., is more than the capacity of the solid piece *a e b c f d* by the capacity of the two small cones 0·32 diameter at the base and 0·185 or

[1] The words of para. 10740 up to this point are from the last page of folio volume V of manuscript.

* [10742]



0·19 high. So the whole capacity $3{\cdot}3945 - 1{\cdot}4$ or less $= 1{\cdot}9945$ plus the capacity of the two little cones = altogether to 2 cubic inches, may be taken as the capacity of the air chamber *a e b c f d*. On measuring the air chamber with Alcohol, its capacity was found to be just below 2 cubic inches.

10743. Now the Guage used (10731) was rather less than the $\frac{1}{100}$dth part of an inch in diameter—a wire of that size could not pass. If it had moved in the experiments the 100dth of an inch backward or forward, that quantity would have been easily seen and appreciated. Such a cylinder, moving through the space of the 100dth of an inch, moves through a space which is less than the millionth part of a cubic inch—in fact through a space equal to ·0000007854 of a cubic inch. Now this is only ·0000003927th of the space of the air chamber—or $\frac{1}{2,546,473}$rd part of it. If therefore the difference between any of the gases used, as for instance oxygen and nitrogen, had been such as to produce expansion or contraction to this small amount, namely a $2\frac{1}{2}$ millionth part—it could have been seen at the guage. So that I think, under the circumstances, I must conclude there was no expansion or contraction equivalent to the repulsion of the gases, nitrogen, etc. in oxygen or air which were observed in the former experiments.

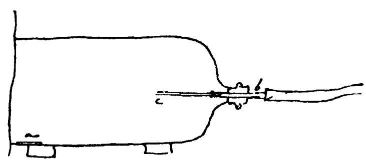
10744. Is, then, the effect an effect not of attraction or repulsion, but a differential effect of another kind between the two bodies which are free to go to the pole? (10786).

10745. I must see what expansion of air (as by hydrogen) will cause a current sent horizontally to pass upwards with the same amount of force as the currents of gas do travel with in the magnetic field.

28 JANY. 1850.

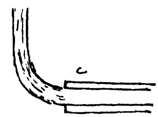
10746*. Arranged a jar horizontally, closing up the large end by a plate of glass and adjusting a cork and tube at the mouth. The tube was connected with an apparatus supplying about 6 c.i. of gas of any given kind per minute (9138, 45, 8, 54). A piece of paper moistened with ammonia was laid in the jar at *a*, and another piece of paper moistened with sol. Mur. Acid was put into the tube at *b*. The air delivered at *c* was made beautifully visible in its course by the formation of a cloud, and not merely was its course

* [10746]

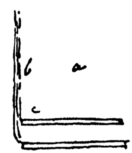


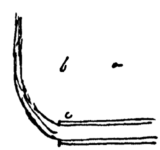
made visible, but also the production of fine slow accelerating undulation spreading thrgh. the jar. The tube *c* was of glass, cylindrical and 0·15 of an inch internal diameter, so that the stream of gas issuing from it was 0·15 of an inch in diameter and its velocity that due to the delivery of 6 or 7 cubic inches in a minute, therefore slow.

10747. When Air was in the Jar and also delivered by the pipes *b*, *c*, etc., the heat of combination of the M.A. and Ammonia was such as to make the issuing horizontal stream rise upwards with about this curvature and ascend at least 3 inches until it struck the top of the jar and then rolled on. The stream preserved nearly the same width the whole way up to the top. So a very little temperature, and therefore a very little expansion, is able to divert the air from its horizontal course and give this deflection on one side of that course. I doubt if in any of the cases of gases at the same temperature, as of oxygen compared with nitrogen or coal gas, there was more deflection from the upwd. course of the lighter gas than this.



10748. Now sent in Hydrogen by the tube *c*. It gave the visible cloud but it turned up at once from the tube *c* and it was quickly wire drawn into a much smaller diameter than that it had on leaving *c*. The force of ascent by gravity was very manifest. Whether in an atmosphere of oxygen, the power of the magnetic axis would be such that, if first at *a* and then advanced on to *b*, it would deflect the stream there and send it as far off from *b* as *b* is from *a* in the figure (with these dimensions[1]), I do not know; but I suspect not. Still, if it would, it would supply a measure of diamagnetic force in relation to the force of Gravity and so enable one to calculate the expected expansion (10753, etc.) 10751, etc.





10749. Assuming then that such would be the case, and assuming *b c* as half an inch, then the diamagnetic force of *b* on hydrogen at half an inch distance would be equal to the ascensive force of a column of hydrogen half an inch high in oxygen, which would be measured by the difference in weight of two columns of oxygen and hydrogen half an inch high. If this be calculated for columns having bases of a square inch, it comes to the difference of weight

[1] Drawing is $\frac{3}{4}$ scale.

of half a cubic inch of each of these gases. Now half a cubic inch of oxygen weighs 0·1719 of a grain, and half a cubic inch of Hydrogen weighs 0·01074 of a grain, and the difference is 0·16116 of a grain, and may be considered as the expression of the diamagnetic force outwards upon a square inch of surface at that distance from the magnetic axis. Now 1 atmosphere of pressure on a square inch = 105,000 grains, and this divided by 0·16116 of a grain gives 651,526, so that the diamagnetic force is only equal to the $\frac{1}{651526}$th part of an atmosphere, and could only produce an expansion to that amount.

10750. It is true that the parts nearer to the Magnetic axis would be expanded to a larger amount, and those further off to a less amount, and as my chamber (10730) had a diameter of 1·7 inches, I may perhaps assume for the sake of something that the mean expansion in it might be $\frac{1}{651526}$th part of the capacity; i.e. that difference between the oxygen and hydrogen in it might amount to this quantity. But then my magnetic axis is occupied by iron (10730), and I doubt much whether with it and my magnet force a stream of hydrogen in oxygen could be deflected to the amount above given. If it could, then as my guage could tell by very careful observation a difference equal to $\frac{1}{2,546,473}$ of the capacity of the chamber, it ought to be able to shew the difference between oxygen and hydrogen—though that difference is very small (10743).

28 FEBY. 1850.

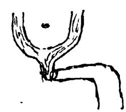
10751. Proceeded to deliver a jet of hydrogen underneath the magnetic poles in contact, the hydrogen being delivered at a certain distance below the poles (10748); for which purpose the great Electromagnet was used, urged by 20 pairs of Grove's plates, and the terminal pieces were those running upward (9231), which were covered with the oval French shade (9231) as before, but standing on a basis of vulcanized Indiarubber which fitted elastically round the pole piece and the tubes carrying in the gases so as to insure that the atmosphere within the shade kept its own pretty well. When Oxygen was made the atmosphere, it was sent in from an Air holder as on the former occasions (9231). The hydrogen was also sent in as before, from a Woulf's bottle and

stream of water passing into it (9158), and the latter was so regulated that 6 cubic inches passed in a minute, when the cocks were opened (9159). Paper moistened with strong solution of Ammonia was put into the french shade to make the oxygen atmosphere sensitive—and paper moistened with strong solution of M. Acid was placed in the hydrogen tube (9232) to render its course evident.

10752*. When the experiment was made with air in place of oxygen, the French shade, etc. was used exactly as just described, but common air was left in it—in place of oxygen.

Now the form and size of the poles were as figured below[1]—the two hemispherical pieces screwing into the heads of the rising pieces. The hemispheres were brought into close contact to represent as nearly as might be the former shape (10742), and the place of the end of the hydrogen jet below is shewn. This jet threw out the hydrogen jet in a horizontal direction, that now impetus from momentum might be added to it and that the result might compare with the former result (10746).

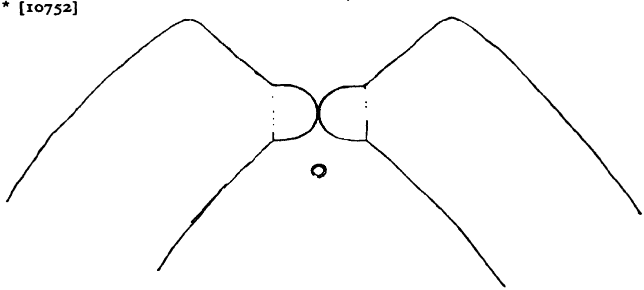
10753. Now when the magnetic force was not on, the stream of hydrogen rose directly up from the end of the tube as on the former occasion (10748), both in air and oxygen, as was to be expected. But when the magnet was thrown into action, then the stream, if exactly under the joint axis of the iron hemispheres, divided beautifully, forking off into two portions which rose one on each side of the axial lines. These portions tended to spread a little parallel to the axial line, but if very central, the limbs of the fork were very defined and compact.



10754. When the limbs of the fork had risen as high as the level of the axial line, then they did not open out further but continued their quick course upwards in two upright streams. When air was in the glass shade or chamber, the fork divided beautiful[ly] upon first putting on the stream of gas and then exciting the magnet, and the division descended to about 0·3 or 0·32 of an inch below

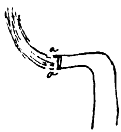
[1] Drawing is $\frac{3}{4}$ scale.

* [10752]



the level of the axial line, making a perfectly clear round space as above shewn (10753), and the distance or radius from the axial line to the bottom of the fork was sensibly the same as the radius on the two sides level with the axis.

10755. When oxygen was in the glass chamber, then the division descended farther down, until it took place directly at the delivery tube, or 0·55 of an inch below the axial line, and the two limbs of the fork opened out farther apart from each other in proportion. This was as expected, because of the greater magnetic or diamagnetic difference between Hydrogen and oxygen than between Hydrogen and Nitrogen (9249, 50, 1).



10756. Look at the matter in this way. Suppose that the Magnet can send a stream of hydrogen 0·1 of an inch in diameter in a horizontal direction (10754), as it seems able to do at the distance of 0·55 of an inch below; the diamagnetic force at that distance being equal to the force of gravity of oxygen minus hydrogen at the place *a*, i.e. of columns of the respective gases 0·1 of an inch in diameter.

1 c.i. of hydrogen = ·021483 of a grain
1 c.i. of oxygen = ·343728 of a grain

·322245 is therefore the difference of weight, and if 1 c.i. be expanded into a layer of 0·1 of an inch thick and 10 square inches in extent, then this difference represents the buoyancy of such a layer of hydrogen in oxygen, and may be compared to the pressure of an atmosphere thus by comparison of a square inch in the two cases. As ·322245 represents the difference in gravity of 10 square inches, so ·0322245 will represent the difference of 1 square inch. Now 1 atmosphere on a square inch equal 105,000 grains or 15 lbs., and this divided by ·0322245 gives 3,258,390 nearly. So that the ascensive power or flexion upwards of a horizontal stream of hydrogen 0·1 of an inch in diameter or vertical thickness (as at *a*, *a* above) in oxygen is $\frac{1}{3,258,390}$ of an atmosphere.

10757. So if the deflexion, by a magnet, of hydrogen in oxygen be, as it seems to be in my last experiment (10754), enough to make the stream go horizontally, counteracting the force of gravity at that distance, that force converted into pressure in a close vessel would give an expansion due to $\frac{1}{3,258,390}$ of an

atmosphere, which is very small. But I think I ought to take not 0·1 but 0·55 as the depth of the column of hydrogen which the magnetic force could counteract in respect of its tendency to ascend in oxygen, for there can be no doubt I think that a column, extending from the axial line to 0·55 below it, would be sent off horizontal. In that case, $\frac{1}{3,258,390} \times 5\cdot 5$ gives $\frac{1}{59,243}$ [*sic*] as the amount of expansion produced within that radius. Now as my guage permits the easy observation of an expansion of $\frac{1}{1,000,000}$ (10743), I think I ought to be able to observe such an effect when the iron core is more turned away (10742, 10764), and a stronger magnet is used.

10758. Of course, if oxygen should prove to be magnetic, then I cannot expect to observe such a difference at once, because it would be divided into two portions, being contraction for oxygen and expansion for hydrogen; but this would only make the subject the more important. I must examine the pharmaceutical Magnet.

10759. As Nitrogen is more diamagnetic than hydrogen, so it will be the best gas to compare with Oxygen. Try Carb. acid and Nitrous Oxide at the same time.

14 MAR. 1850.

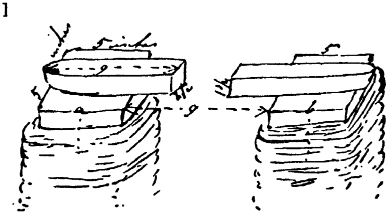
10760. Have made *Five Guages* like the former in nature (10715). They are marked and have nearly the following diameters in the narrowest part. The marks are file marks on the cap:

— $\frac{1}{150}$ of an inch—not more.
= between $\frac{1}{150}$ and $\frac{1}{200}$ of an inch.
≡ $\frac{1}{200}$ of an inch nearly.
⊁ rather more than $\frac{1}{100}$ in diameter.
⋇ close upon $\frac{1}{100}$ in diameter.

26 MAR. 1850.

10761*. Worked at the Pharmaceutical Society with their large magnet, which is in shape like our large one (   ) but larger. The dimensions are as set down. The distance between the inner edges of the limbs is 9 inches and the median line from *a* to *b* along the core is 50 inches, so that the soft iron is equal to a square bar 5 inches thick and 50 inches long. 1500 feet of No. 7

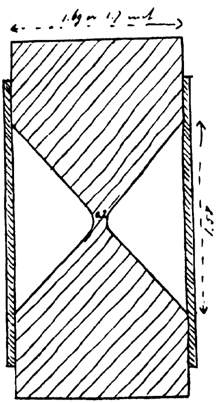
* [10761]



copper wire, being 0·175 of an inch in diameter, is coiled on it in four coils which are connected as one long coil or wire. The moveable terminal pieces are $2\frac{1}{2} \times 1\frac{3}{4}$ inches square and 9 inches long, being planed square at one end and round pointed like ours at the other ends.

10762. We had 80 pair of Grove's Plates the size of ours to connect with this magnet. I first connected them as four 20, so that the battery now consisted of 20 pr. of plates of fourfold size. When this was on the magnet, a large magnetic needle at a certain distance, about 5 feet, made 11 vibrations in 15 seconds. I then connected the battery as two forties, i.e. 40 pr. of plates of only two fold size, and now the same needle made 10·5 vibrations in 15″.

10763. The needle was now placed nearer and with the last arrangement gave 20 vibrations in 10 seconds. The battery was now arranged as 20 pr. of plates of fourfold size and the same needle in the same place gave 22 vibrations in 10 seconds. So we kept this arrangement of the battery for the future experiments.



10764. The box apparatus (10730, 2, 42) was now adjusted as before. It was in the same state as before except that I had had a portion of the iron turned away from the middle, so that its form and dimension is now as given and not as at (10742). Its capacity is now 2 cubic inches (10854). Also I used one or other of the guages described (10760). The Guage of $\frac{1}{200}$ of an inch internal diameter was first attached and a microscope of low power mounted over it for the observation of the fluid, which was alcohol coloured with cochineal as before.

10765. It is important that the coloured fluid be in *one* short portion, and that there be no other or diaphragm portions elsewhere, for they seriously obstruct the motion of the fluid in the tube (10775). Also it is important that the tube be wet on both sides of the place where the fluid naturally rests—or else it will not travel with a little force. This effect is easily obtained by holding the guage vertical with each end upwards before it is attached to the gas box.

10766. *Oxygen.* Introduced this gas as before (10714, 9, 23, 34, 10773, 6) into the enclosed magnetic field in the dry state. Shut off supply—opened air cock for a moment and then the guage cock—and found the indicating fluid to occupy its appointed place

very well. All motion from change of temperature was prevented by the use of the many fold flannel jacket which surrounded the apparatus (10732).

10767. On making and breaking contact, there was an effect at the guage. The effect was that of expansion on making contact and return or contraction on breaking contact—but the effect was very small and seemed to come on or be shewn with difficulty.

10768. Removed the present guage and put on one of $\frac{1}{150}$ of an inch in diameter (10760). There was a clearer result of motion than before but the same in kind—expansion by contact of battery and return or contraction by breaking contact. The apparent amount of motion was about equal to half the diameter of the indicating fluid, but then that diameter is seen increased by refraction.

10769. *Nitrogen* introduced and the $\frac{1}{150}$ of inch guage. The effects were exactly as with *Oxygen*, neither more nor less—the same in direction and in all things. Yet Nitrogen and oxygen very different indeed in their diamagnetic relations (10720, 34).

10770. Thought that these fine guages presented obstruction to the motion of the fluid within them, and so removed that which was on and replaced it by a good one of $\frac{1}{100}$ of an inch in diameter, the *Nitrogen* still being in. This gave a better result than the former guages though exactly of the same kind. Could now perceive that the expansion on making contact required some time to attain its maximum, perhaps two seconds, but the contraction on breaking contact was more sudden. This is probably due to the gradual rising of the magnet in power and is parallel to the effect produced by the coming on and passing off of the rotation of a ray of light (   ).

10771. I think also that on making and breaking contact 3 or 4 times in succession the expansion (apparent) is a little more than the contraction.

10772. *Nitrous oxide* and the $\frac{1}{100}$ guage. Same as Nitrogen—the same amount of slow expansion and quicker contraction. I could see no difference (10722, 34).

10773. *Oxygen* again, and the $\frac{1}{100}$ guage (10760). Same as the two former—no difference. The Guage acts very well and better than the smaller guages (10714, 9, 23, 34, 66, 76).

10774. *Carbonic acid gas* and the $\frac{1}{100}$ guage. As oxygen, nitrogen and nitrous oxide, both in direction and amount (10721, 34).

10775. *Air* and $\frac{1}{100}$ guage. As before, i.e. as the last four—in direction but less in amount (10717, 34). But I think this is due to a little diaphragm film of indicating fluid which has gathered up apart from the indicating portion and offers some resistance to motion (10765). So introduced

10776. *Oxygen* again and the $\frac{1}{100}$ guage as in the last experiment and found the effect as nearly as possible the same as the last (10714, 9, 23, 34, 66, 73).

10777. There is no appearance therefore of difference amongst these gases, though they differ very much from each other in their diamagnetic force; and the effect that does occur is probably not due to the gases but is some general effect of the apparatus.

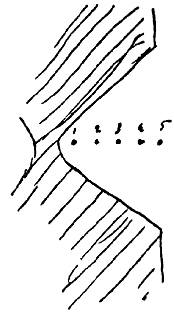
10778. It may be due to a real compression of the iron between the poles of the magnet when the force is on; for such an effect would produce the apparent expansion by diminishing the capacity of the air chamber (10742). The magnet appears to spring together when the force is on, for then the iron core is very tightly griped between the two terminal pieces. But when the Voltaic battery is thrown out of connexion, then shortly it becomes loose between these pieces and appears not to rub, as if they had very slightly receeded. Must try the effect of ordinary pressure (10853).

10779. I then took off the copper outside from the core (10730), so as to observe the diamagnetic effect exhibited there by a candle flame or a smoking taper with this magnet and power of battery, and found it very strong and good—equal to what I expected. There seemed to be no deficiency of diamagnetic power to account for the non-effect in the inclosed magnetic field (10736).

10780. I also put up my two hemispherical poles (10752) and found the effects with them as good as I expected.

10781. *Considerations* (10792). If the diamagnetic force in gases be a force tending to make each particle go from stronger to weaker places of force, then surely it ought to cause expansion in such an apparatus as that employed. For consider only the particles in a radial line, i.e. in a line transverse to the magnetic lines of force; particle 3 for instance—it is naturally kept in its place by the elasticity of the particles 1, 2, 4, 5, etc., the external

one being of course governed by the pressure of the atmosphere. But now, if a force be superadded to particle 3 tending to urge it towards 4, it will surely push 4, 5, etc. outwards and will receed from 2 until the original separating elastic force exerted between 2 and 3 is diminished by an amount equal to the outward diamagnetic force of 3—for the elastic pressure of 4 on 3, which was before sustained by an equal elastic pressure between 3 and 2, is now sustained in part by the outward diamagnetic force and in part by the elastic force between 3 and 2 diminished (by distance) to an amount equal to the outward diamagnetic force. For the same reason the distance between 2 and 1 and also between 1 and the core must be diminished in the same degree; and indeed all the particles between 3 and the core are relieved by the outward diamagnetic force of 3 and therefore must tend to expand.



10782. But what is true of 3 must be also true of 2 and again of 1, and so the distance between the iron and 1 must be diminished by the outward diamagnetic force of 2, and again by that of 3, and again also by that of 4 and of 5. In fact, the diamagnetic force of any particle must, upon the assumption above (10781), relieve the elastic force of all those within it and push outwards all those without or outside of it. For all the particles are subject to each other by their mutual elasticity, and the diamagnetic force is one (by the assumption) which tends to make every particle move outwards.

10783. But then why is there not expansion shewn in the close vessel? (10792, 10796).

10784. One can hardly suppose that it resulted from the tendency to expand being in one direction only and not in all directions. It is true the direction of expansion is in the lines of the particles 1, 2, 3, 4, 5, and not in the direction perpendicular to this. Now such a direction of the expansion makes every line of particles abut on the inner surface of the copper vessel, except that opposite the aperture of the guage, and at first it might be thought that the tendency to expand in this direction was resisted by the rigid vessel, and so the expansion not allowed to come into play. Still, though it be true that the force of expansion is in lines outwards, it seems almost impossible to resist the idea that this must be conveyed on in lateral and all directions (hydrostatically) in a

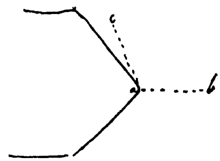
fluid so mobile as air or gas, and so the whole expansion find room to exert itself at the guage and make itself visible. Suppose the air in the chamber be divided into 100 strata concentric with the copper chamber and each other; then if the inner 50 tend to expand diamagnetically, they would surely have that tendency conveyed to the guage by the outer 50 if they *were not* in the diamagnetic state–and their being at the same time in the diamagnetic state does not seem to give any reason why they should not also effect the same purpose; then surely the elasticity of the *outer* strata of the enclosed gas must be able to convert the radiant force of the inner strata into expansion of the whole. If one could conceive the air of the magnetic field rapidly revolving, it would tend to expand by centrifugal force in the same lines of direction as by diamagnetic force, and there seems no reason why the expansion should not in both cases tell at the inner mouth of the guage aperture or tube.

10785. The relief of elastic force by the supervention of the diamagnetic force is only in one direction, i.e. radially or outwards; but there appears every reason (from the principles of hydrostatic pressure already touched upon (10784)) to believe that the expansion would take place in all directions, i.e. that the particles would not merely separate in the direction of the line of diamagnetic force but in every direction. Else the assumed counterpoise (10784) could not occur.

10786. But the Magnetic power is a *polar* power and the diamagnetic power may be *polar* also, and then the above conclusions do not necessarily follow (10744). It is not impossible that whilst the gaseous particles tend to separate in the direction of the radius, they may tend to approximate in the direction of the line of magnetic force; and then if these two contrary actions were equal in power, they would leave the whole mass of air affected unchanged in volume–though perhaps tending to motion. However, I see no reason for this view at present, except that it would explain the no change of volume. But then surely other effects of such a condition should follow and be recognizable–as for instance refractive power in different directions, etc. etc.

10787. In relation to the gas in a diamagnetic field (unenclosed): if one part be hot–then there are currents produced (   );

or if one part be different gas to another part, as oxygen and nitrogen, then there are currents ( ). But if the gas be all alike and all at one temperature, are there then currents outwards in the direction of diamagnetic action? It would seem as if there ought to be, unless the diamagnetic force diminished in the same proportion in every direction from the central part of the magnetic field. Now supposing a conical pole, as in the figure: one would think the decrease of magnetic force more rapid along the line *a b* than along the line *a c*, and therefore that in a uniform gas there would be a tendency for it to move from *c* to *a*, and from *a* to *b*, i.e. an ultimate tendency. If there is no such tendency, then how is the gas in the lines *a b* and *a c* preserved in equilibrium? The mere force of tendency from the stronger to the weaker places of action would not seem to be enough (10735, 6, 7, 8, 9, 10792, 10798, 10856).



10788. Then, also, what is the relation either as to repulsion of the hot part, or as to mutual action of the hot and cold parts of a same gas, and why are they unstable—and present currents, even currents downwards and against gravity? I mean, what are the true physical conditions of the hot and cold parts, particularly in reference to each other, under the magnetic influence?

10789. Also as to different gases: they have the same relation to each other at like temperatures as the same gas has at unlike. Do they move in each other by some other force or principle than the mere amount of diamagnetic repulsion? Is there any relative conjoint cause of their motion? It does not seem likely that *masses* of different gases should bear opposite polar relations to each other (10797).

10790. Suppose an atmosphere of nitrogen in the magnetic field and a stream of oxygen passing across it; the diamagnetic difference would make the oxygen approach to the magnetic axis. But if the stream of oxygen were heated to a certain point, it would then have exactly the same diamagnetic relation as the nitrogen—or if it were more heated would be more diamagnetic. What is the real magnetic relation of the oxygen and the nitrogen in these their different relative states?

10791. Having the advantage of the Pharmaceutical Magnet and

battery, I tried an experiment on a ray of light passing equatorially through heavy glass. Two terminal pieces of iron, shaped as in the figure* so as to have blunt edges that could be placed against a long piece of heavy glass polished at the ends, were arranged as in the margin[1]—the edges and the piece of glass being 2 inches long. Then a ray of either blue, or red or yellow light was sent from *a* to *b* and observed by the eye at *b*, the object being to observe whether there was any *momentary* change either of *colour* or *intensity* at the instant of putting on or off the magnetism. But nothing of the kind or of any other kind could be observed.

4 APRIL 1850.

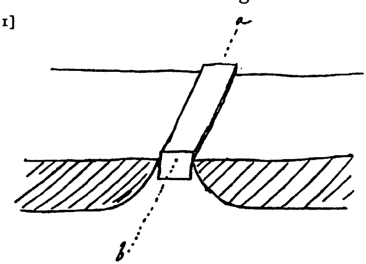
10792. In reference to the cause why one gas approaches to and another receeds from the center of the Magnetic field (10781, 3), and why there is no motion if one gas only be present (10787), it must be remembered that the direction in which the two gases are urged is doubtless a differential action, just like that of two solution[s] of iron of different strengths one in the other (   ). Exp. Researches.

10793. Now the position of the masses of substances in that case may be referred to the relative power of the bodies to *conduct* on the magnetic influence in the line of magnetic force, and a body not a permanent magnet may be considered more magnetic than another according as it allows or favours the transition of more magnetic force through the same amount of space. The media may be considered as conductors of magnetic force: and that which can conduct the most will of necessity be drawn into the place of most intense action; that which can conduct the least will be drawn out of the place of more intense action by the former.

10794. In this case, it is not true that the one is attracted and the other repelled, but that both are attracted but with different amounts of force, and the better conductor occupies the place of greatest force.

[1] I.e. as in the figure.

* [10791]



10795. So if two gases, as oxygen and nitrogen, have different amounts of conducting force, then that which has the greatest will go to the most intense place of force (the oxygen), and that which has the least (the nitrogen) will go outwards to the least place of force. The motion of the two is a differential result, both being in *relation to* and *dependance on* each other by being in relation to the Magnetic force which passes through them.

10796. The passing outwards therefore of the nitrogen should not indicate any probable expansion therefore of that gas when alone; for it does not tend by mere repulsion to go outwards, but on the contrary may tend inwards if a stronger conductor were not there to repel it. Hence there is no reason in that view to look for enlargement of its bulk (10783), or indeed any change in that respect.

10797. Hence the reason why the masses can affect each other (10789), without being liable to expansion or contraction.

10798. Hence the reason why one gas is in equilibrium and has no currents formed in it (10787), for then the substance that is in any given place can conduct as well as any other part of the substance that is free to go there.

10799. The force which urges a body (as oxygen) to the center of the Magnetic field or force is not like gravitation pressr. (which would compress) or like attraction (which would condense). It is not a central but an axial Magnetic force, which seems to have no power of drawing the particles closer together.

10800. What is the effect on iron when magnetised as to change of volume? Try glass bottle full of filings and water and a very delicate Guage–Joule; also (10857, 8).

10801. This must establish a distinction between the kind of force drawing the oxygen to the intense part of the field and anything like mere attraction–for a mere attraction to the center of force must I think have caused a sensible condensation of volume; yet no such occurs.

10802. It would probably not be difficult to state all the effects of gases by saying that they are repelled, but some more than others. Then those which go inwards, as oxygen, would do so only because the others, as Nitrogen, are more repelled and it, the oxygen, less. This is simply reading it the other way. But

as respects the results of experiment, as there is no sign either of expansion or condensation in any of them, so the effect does not appear to be one of mere attraction or repulsion. It is probably due to the assumption, by the better conductor, of the place of greatest magnetic force and neither an attraction or repulsion.

10803. But then one ought to be able to obtain some evidence in other shapes of effect of these differences in conducting power.

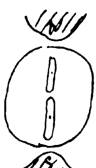
10804. Considering bodies, then, as conductors of magnetic force, the best conductors would converge or draw up, displacing or driving out the worst conductors. But all would converge if not driven out by others—the results being differential (10832).

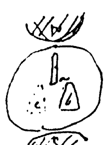
10805. In that case, bismuth and Iron act by the same kind of force possessed only in different degrees, as I supposed long ago (   ). Exp. Res.

10806. But then, what is a vacuum? It must either be considered as a zero, or as a real conductor of magnetic force. In the first case, it would have Iron and Bismuth on opposite sides as magnetic and diamagnetic bodies—or on the same side as both magnetic bodies.

10807. I am obliged still to retain my former suspension of opinion in that respect (   ). Exp. Researches.

10808. But proceeding on the view that all bodies, metals, gases and even a vacuum, is a conductor of magnetic force, then let us consider the case of polarity as it is called; i.e. the polarity of masses being magnetic or diamagnetic *only* whilst under induction, and not that of permanent magnets.

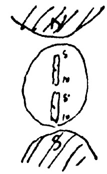




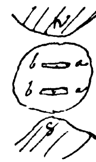
10809. Two cylinders of a diamagnetic body, as heavy glass or common glass, would in the magnetic field set as polar bodies if they were immersed in a fluid medium more diamagnetic than they, as perhaps melted Phosphorous or Sulrt. Carbon (10888).

10810. Would their ends then really affect each other, or would a suspended cylinder *a* be affected in position by another piece *b* placed at *b* or at *c*? (10888).

10811. If so, might reconcile and subject all the cases of induced polarity, whether of magnetic or diamagnetic bodies, under one view. But should still have to work out this relation in respect of the permanent polarity of a magnet.

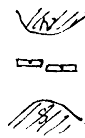


10812. Compare these notions with the polarity in the case of two solutions of iron of different strength. A cylinder of the stronger solution moving in the weaker would exhibit the ordinary magnetic polarity, and two such cylinders would shew the succession of poles, i.e. of points tending to come together that the discharge or conduction of magnetic force may be a maximum (or most favourable position) (10887).



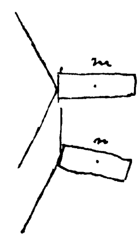
10813. But now, if the cylinders were of the weaker solution and the surrounding medium the stronger, where then would the poles or the corresponding points be found? Would they not stand thus and would not the extremities *a*, *a*, be alike, and also the extremities *b*, *b*, be alike.

10814. There seems however no reason why *a* should differ from *b*, for each is in the same relation to the magnetic axis. Still, suppose them placed thus: it does not seem unlikely that *a* and *b* should be points having a virtual relation to each other in the whole system of conduction of magnetic force from N to S, and be able to affect each other a little. Very little indeed is to be expected.



10815. If so, then the effect might be this: that supposing the two cylinders were suspended by bifilar thread thus, they should upon the supervention of the magnetic force take up this position , each one setting round a little until they were in or approached to a right line (10886, 7, 8).

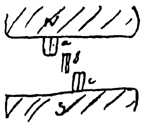
10816. Might use two pieces of heavy glass in air—or to render the case extravagant, might use two such pieces of heavy glass, or of bismuth or phosphorous, in a solution more or less strong of proto sulphate of iron (10876, 11104).



10817. One of the pieces might be a fixture and the other mobile, and then observation could be made by observing an image reflected from the end of the mobile piece; for an object would be seen in one place by reflexion in position *m*, and in another place in position *n* (10876).

10818. Then in reference to Magnecrystallic bodies considered only as conductors of magnetic power, and so only as exhibiting points which may be called poles, as for instance a crystal of bismuth in a field of uniform magnetic action, or in one diminishing in force rather. It should surely be expelled sideways from the

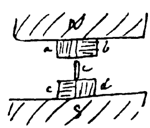
center of the magnetic field more forcibly when the M.C. axis is equatorial than when it is axial? (10888).



10819. Also may not one crystal affect another in the uniform field of action this way? Let *a*, *b*, *c* be 3 crystals of a M.C. body with the M.C. axis axial, *a* and *c* being fixtures and *b* finely suspended; will *b* tend to set round a very little when the magnetic force is on? Or if *a*, *b*, *c*, *d** be M.C. crystals, fixtures, the M.C. axes of *a* and *d* being axial and of *b* and *c* equatorial, will the M.C. crystal *e* swing round a little on putting on the magnetism? (10888).

10820. In considering and comparing different bodies as conductors of Magnetic force, must remember that space or a vacuum appears to be a conductor or have power of transmitting the force across it; as it has also for electrical induction and for the force of Gravity. But then, in considering other media or spaces, as Iron, Glass, Oxygen, etc., must remember that the effect is compounded of the two, space and the body, and that though this may make little difference when iron or Nickel are concerned, it may make much when a gas, as Oxygen or nitrogen, is concerned, where the power of the substance may be very little as compared with the power of the space.

10821. Should be able to measure comparative power of space or a gas by a bifilar suspension of a bar of low diamagnetic power in it.

* [10819] 

10822. Consider Magnecrystallic bodies as conductors of Magnetic force, and look to Plücker's last letter to see how he considers them.

10823. They must be considered as different in conducting power in different directions, and this may very well be the case.

10824. Then there is no reason why such a body may not be above a vacuum or 0° in one direction and below it in another, and so be both a magnetic and a diamagnetic body.

10825. But then, should not such a body shew attraction in one direction and repulsion in the other—or in other words, go from stronger to weaker places of action in one position and from weaker to stronger places in the other—being in a vacuum? Exp. Res. 2551, etc. 2561.

10826. How would this consist with the former experiments on Sulphate of Iron (9950, 1, 23[1], 10006)? Must reexamine and reconsider them.

10827. Also how would non-magnetic M.C. bodies stand, as Calcareous spar? And how would red ferro prussiate of potassa stand (9955, 6)? Would have to see in all cases whether the center of gravity approached to or receeded from the pole in vacuo or its equivalent of gas.

10828. Consider this relation and direction of conductive force in conjunction with the Magne optic and Magnecrystallic force—seems to be very simple and large and now to include crystallization and electric force in one conjunction (   ). Consider also what the relation (if any) might be to Static Electricity (   ).

10829. Also as regards dynamic electricity—are there not some new phenomena and consequences to be deduced, especially in directions at right angles to the line of magnetic force?

10830. As to Reich's experiment on polarity of diamagnetics. Suppose that a cylinder of weak solution of sulphate of Iron were in a surrounding medium of stronger solution; it would point equatorially as a diamagnetic. But would not a piece of iron under

[1] ? 9973.

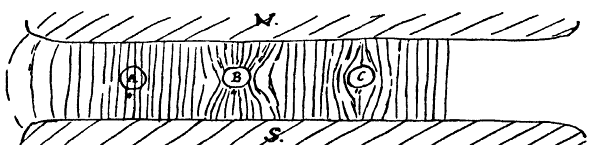
it affect it as it does a diamagnetic body? Yet surely this weak solution cannot be supposed to have contrary poles to what it naturally would have, simply because the stronger solution is around it (10895).

10831. But this makes one think of what is a pole. In Ampère's theory a given pole, N or S, is that end of a line which, going through a ring current, is in a certain constant relation to the direction of the current; or if the current be rectilineal, expands into the Northness on one side of it and the Southness on the other.

10832*. If we confine ourselves however to the idea of "conduction" (10793, 804) and consider what a pole will be under that point of view: then supposing a magnetic field of lines of equal magnetic force, if a body, as A, were placed in that field, having no difference in conducting power from the medium or vacuum occupying the magnetic field, then the lines of force would be undisturbed; the body would be neither magnetic nor diamagnetic, and would present no appearance of poles or any other magnetic phenomena. But if the body B conducted the lines of force, then there would be a gathering up of them on it in the direction indicated: the body would assume the polar state; lines of force would converge upon the polar parts; and this body would be a magnetic body, able and willing to go from weaker to stronger places of magnetic force. If on the other hand the body was a worse conductor of the lines of force than the space, C, then the lines of force would diverge from it, passing by it; and this body would be a diamagnetic substance tending to go from stronger to weaker places of magnetic action.

10833. Now C′ and C″ have a polar relation to A′ and A″, which are non polar, just as strong as that which exists between B′ and B″ with A′ and A″; only the polarities of B and C are antithetical and contrary to each other; for as relates to the lines of magnetic force, they are the reverse of each other. Therefore the polarities B′ and C″ are rather to be considered analogous to each other, and also the corresponding polarities of B″ and C′ may be likened; and indeed, as concerns the directions into which the lines of force are thrown by these polarities, *they are the same* either at B′ or C″ as regards the surrounding system of forces,

* [10832]



whether reference be made to the N or the S terminations of the lines of force, or to any other part; and the polarities B″ and C′ have also the same identity in this respect.

10834. So then in this view, a body conducting Magnetic force better or worse than a vacuum would have contrary polarities (   ), partly according to the views of myself, Weber, Reich and others. But how does this accord with the Ampère polarity (10831), or that of a permanent magnet? To do so we must assume, as it appears to me, that all conduction of magnetic force is carried on by circular electric currents round the line of magnetic force in the whole of its course, and in that case that they must exist in a vacuum itself; which is difficult to comprehend according to the Ampère theory, where the circular currents are associated with the particles, or with any other generally acknowledge[d], or even any proposed view or even any trial speculation that I am aware of. Then Magnetic bodies would be bodies allowing of the induction of circular currents more abundantly than would occur in the space they occupy, and diamagnetic bodies would be those which allowed of the induction of currents less abundantly than in the space they would occupy.

10835. The notion that diamagnetic bodies are bodies having *like* magnetism with magnetic bodies (Becquerel), but less than space, would also require this view of space. For if the lines of magnetic force be supposed to pass otherwise through space and without the existence of these circular currents (and so more in analogy with the manner in which the lines of gravity and of inductive static electricity pass thrgh. space), then it does not seem consistent that a body which permits the formation of the circular currents that can convey on the force should embarras the action of space and diminish the transferring power there, however small its power to sustain these current[s] would be. These currents should rather assist the action of space—and bismuth, however small its magnetic condition should be, ought to add it on to the power of space, which it does not do. It rather opposes it, and as I had speculated and Weber assumes, the effect may be due to counter currents which would then give not a sum but a difference of action.

10836. But then diamagnetics would be anti-magnetic as to iron.

10837. Falling back on the view of conduction, is it not probable and most likely that lines of Magnetic force can be transferred across space in the manner of the lines of Gravitating and Static Electricity force, and without these circular currents (or their equivalents), which are assumed to exist in iron when it is in the magnetic field? In addition to this, is it not probable that some bodies may affect the transmission of this force across space when they occupy it, without having the circular currents set up in their particles either in the one direction or the other, by an act or process which I have endeavoured to express by the word conduction? Then bodies which conduct better than space transmits would favour the transmission and be as the type B, and those which conduct worse than space transmits would be as the type C (10832). The latter would be diamagnetic bodies, but the former would hardly take place as Magnetic bodies, though they would in respect of conduction or transmission of force act in like manner with them.

10838. Then the Polarities before referred to (10834) would be a true polarity of conduction and would present the *opposite* and *antithetical* conditions, and their relations to optical polarity would probably grow out and expand much. But they would not present cases of true magnetic polarity, according to the Ampèrian view of the matter (10831).

10839. In that case it might be that cold Iron, Nickel and cobalt are magnetic bodies; but that when heated they are not so, but have entered into the condition of conductors better in power than space.

10840. All bodies which could *retain* a magnetic state would of course be true Magnetics. Likewise all bodies which for the time (whilst under induction) could assume the same condition would be true magnetics. But bodies not assuming it would be the class of conductors.

10841. *How shall we discover and establish an experimental and physical distinction between the class of magnetic and that of conductor if it exists? Or how shall we prove* EXPERIMENTALLY *that they are physically the same, if there be no such natural distinction?*

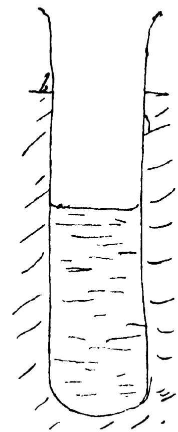
10842. The phrase conducting power, when used as a general expression and implying nothing as to how the process of

conduction is carried on, is very useful; for it enables one to take a connected, consistent and general view of a very large class or phenomena, serves as a standard of meaning amongst them, and does not involve any error, inasmuch as whatever the cause of conduction the phenomena must consist together amongst themselves.

10843. What is the best form of apparatus to give a field of magnetic force rapidly diminishing in power *axially*? The opposing cones ought to be pretty good—but then would not this* be better, or this†; or perhaps better than all, two similar poles of opposite electromagnets. The pointed cones or the hour glass core should give a very rapid diminution of force *equatorially*.

16 APRIL 1850.

10844. Expts. in expulsion of bodies from water by freezing. Thought that if I stirred the water or solution continually that I should disentangle the air and concentrating solution, and obtain a clear pure lining vessel of ice. So had some thin wide tubes of glass prepared and put the solution into them, and then the tube into a strong freezing mixture, and with a large feather stirred vigorously the contents of the tube. In from 2 to 3 or 4 minutes an experiment was complete and excellent, as thus



10845. *Blue solution of Sul. Indigo* up to *a*, the freezing mixture being up to *b*. Ice *lined* the vessel up to *b*, because of the agitation made by the feather. In two and a half minutes turned out the remaining blue liquor, which was much concentrated, and then washed the tube with distilled water. It was lined with colourless clear ice from the bottom to *a*, and from *a* upwds. to *b* with more porous ice, and that was therefore a little blue. On warmg. the tube the inner ice tube turned out beautifully and shewed the expulsion of the colouring matter.

10846. As the water freezes, floating flocculent ice forms in the fluid, and it is this which by the feather is thrown to the upper part and chiefly freezes there. On filling a tube with blue liquor up to *b*, i.e. as high inside as outside, then the whole of the ice tube formed was good and very little coloured.

10847. I used (and it is fair) some water from *thawing ice*, to have it free from air, and the expedient is good. The feather action is

* [10843] 

† [10843] 

very good, but it must be kept constantly moving or it will freeze to the side.

10848. *Dilute Sulphuric acid*, strong enough to affect sensibly litmus paper and give a free precipitate with Muriate of baryta. The tube $\frac{2}{3}$rds full. When rather more than $\frac{1}{2}$, perhaps $\frac{2}{3}$, were frozen, turned out residual fluid A. Rinced the tube and contents with distilled water–melted out the ice tube–cut it in half so as to divide the clear part from the upper more opaque part. Melted the clear part into fluid B–the opaque part into fluid C–the original fluid being D. By Mur. Baryta there was no trace of S.A. in B–only a little in C–and very much in A, surpassing that in the original fluid D. Again–D was slightly acid to litmus paper–B and C not at all and A very acid. A little blue cabbage liquor poured into B was not affected; in A turned strongly red. Results very good.

10849. *Sol. Common Salt.* Strong enough to give fair precipitate with Nitrate of Silver. Then the fluid B gave no precipitate, C gave a little precipitate, A gave a strong coagulum. Good, though not so good as Sulc. acid.

10850. *Caustic Potassa.* Solution just able fairly to affect Turmeric paper. The ice was not transparent but fissured and translucent. Nevertheless the effect was good. Turmeric paper shewed the great difference between D and A which was very alkaline; and reddened litmus paper shewed that B was quite free from alkali.

10851. *Ammonia.* Solution able freely to affect Turmeric paper. The ice was very transparent and good and the difference between the concentrated solution A and the liquefied ice B excellent, both by Turmeric paper and reddened litmus paper. Also pouring a little red cabbage liquor (strong) into the fluids A, B, C, etc. gave an excellent result.

10852. *Ice explosive* (as water becomes free from air). Boiled some clean sweet oil in such a tube (10844), and let it cool. Put a piece of pure clear ice into it which sank to the bottom. Placed the tube and contents in an oil bath, and heated by a gas burner carefully. After a while the water in the tube began to bump and burst, and at last drove the tube and its contents out of the oil bath. I have no doubt I could easily obtain an explosion.

10853. Arranged the core and Jacket as on the 26th March, with a guage of $\frac{1}{100}$ of an inch diameter. Then put the core in a vice to ascertain whether pressure longitudinally would affect the guage (10778). Found that it did and that a strong grip in the vice sent the liquid of the guage outward, and that on relaxing the pressure the liquid returned. So the effect of pressure evident.

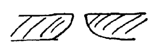
10854. Guaged the chamber by filling the cavity with diluted spirit and then letting the fluid run out and measuring it; it was rather more than 2 cubic inches (10764).

31 MAY 1850.

10855*. I thought as I looked at birds flying that I perceived their image more immediately in the direction of the axis of the eye than in a direction oblique to that axis or sideways; i.e. whilst observing the passage of the same bird in a mirror and directly through a window, the direct and reflected image struck me as appearing at *different times*. I therefore experimented thus. An assistant held in his hand a mirror so covered with paper that only a line half an inch broad was clear, and this reflected a horizontal line of light which by motion of the mirror could be moved rapidly up and down. I then stood 10 or 12 yards off and held a small reflector close to one eye, so that I could, by looking into it with that eye, see in the *axis of vision* the flash of light from the mirror and also see *obliquely* by the side of the eye the same flash directly; the direction of the two rays on entering the eye being at about an angle of 45°. But on careful observation the two appearances or images appeared to occur exactly at the same moment—no sensible separation *in time* occurred, and so my rough observation is corrected and negatived.

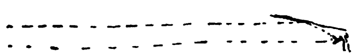
24 JUNE 1850.

10856. Excited the Great horseshoe Electro magnet by 20 pr. of Grove's plates and sprinkled lightly lycopodium dust into the magnetic field, but could see no motion in it indicative of any tendency to currents in the air (10787).

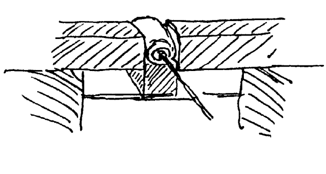


10857†. A small glass bottle (capacity 2 cubic inches) was filled with iron filings and then a guage (10715) fitted by a cork into its mouth and the bottle surrounded by three or four thicknesses

* [10855]



† [10857]



of flannel. It was then placed between the terminations of the magnet so as to be in the magnetic field but protected from any pressure by the poles. The indicating column of fluid was very delicate, but *no effect* of making or breaking contact could be perceived.

10858. The bottle was then filled with Alcohol and the iron replaced, the filings being poured in through the alcohol. The guage was restored and now the whole was a vessel filled with iron and alcohol. Still no effect of magnetism was produced. It is true that at first the iron within was drawn towards one pole and by distortion altered the form and capacity of the bottle; for on making the magnet active, the fluid retreated as if the contents of the bottle had become smaller or had cooled, and on intermitting the current the fluid returned. The effect was very small and did not occur when the bottle was not allowed to press against one side or pole (10800).

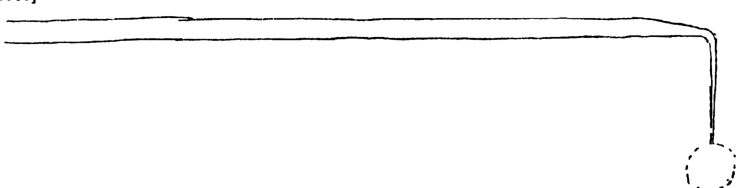
10859. *Bismuth.* Repeated the same experiments with bismuth, both with air and alcohol in contact. There was no change of volume.

10860*. *Soap bubbles of air and gas.* I have endeavoured to ascertain the relation of different gases to each other in the magnetic field by blowing a bubble of gas at the end of a delicate pipe, and then submitting it to the magnetic force in another gas. A glass tube was drawn out to about the size and form of the drawing[1] and fitted at the thicker end to a cap, stop cock and bladder to supply by pressure any given gas. A little thin soap suds was made at the moment by putting a few cuttings of soap into a little distilled water and shaking them for an instant. Old soap suds or that made with warm water is not so good as this, being thicker and stringy. Then after drawing the soap suds up into the tube and also washing the outside of the termination with it, it was easy to make soap bubbles, beautiful in form and more or less ballasted by water beneath, and very mobile in a pendulous fashion because of the smallness of the neck where they adhered.

10861. For instance, if the beak was dipped about $\frac{1}{4}$ inch into the solution of soap, it generally took up fluid reaching $\frac{1}{4}$ or $\frac{1}{3}$

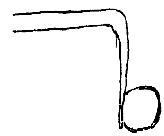
[1] Reduced to $\frac{2}{3}$ scale.

* [10860]

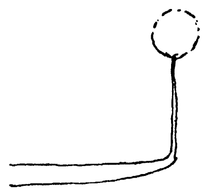


of an inch up the tube. When by pressure on the bladder this was forced out and a bubble of air formed, about the size of that in the figure, such a bubble hung well, but it swung as a pendulum quickly because of the weight of water below and in the envellope itself, and would not be so sensible to any lateral force as a bubble made with less water.

10862. To make one with less water, all that was necessary was by pressure on the bladder (very slight) to force the fluid down the tube and remove it by touching it with the rod used to stir the soap suds, leaving only about the $\frac{1}{16}$ or $\frac{1}{20}$ of an inch in depth instead of $\frac{1}{5}$ or more. Then on blowing the bubble, it was very thin, had little or no ballast water at the bottom, moved slowly as a pendulum, was very easily deflected to one side; kept its suspensory state with difficulty—but rather tended to run up on one side and adhere to the tube, but then could move round it as an axis, only not so easily as when hanging below.



10863. If the tube was turned up thus—then the bubble was always thin, but could not be made to continue upright unless it was very small or unless the end of the tube for it to stand upon was not larger. If of the size depicted[1], the smallest lateral influence caused it to turn down and adhere to the side as before (10862).

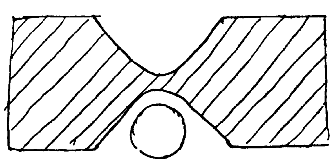


10864*. Now blew a bubble of air, dependant but thin and not heavy with water, and brought it into the magnetic field. When the magnetic force was on, the bubble was determined outwards slightly; and when the magnetic force was taken off, the bubble returned to its first place. This effect I believe due to the diamagnetic condition of the soap sud envellope.

10865. *Nitrogen.* A corresponding bubble of Nitrogen gas was sent away more powerfully than the bubble of air. For here the diamagnetic force of nitrogen above air coincided with the diamagnetic force of the aqueous envellope, and hence the effect was comparitively greater. When the bubble was on the side of the tube (10862) it was very pretty to see how it wheeled round and away, tending to go outwards from the magnetic field. A bubble about an inch in diameter is a very good size for the experiment, and often when it is hanging, the diamagnetic force throws it out and then it runs up and cleaves to the side. By alternating the

[1] Reduced to $\frac{3}{4}$ scale.

* [10864]



contacts with the battery so as to make them accord with the swinging of the bubble when heavy, a very excellent effect is produced (9130, 62).

10866. *Oxygen.* Here the effect was beautiful, the bubble being pulled inwards with very considerable force in Air—and looking exactly as if the oxygen were highly magnetic. This was of course expected and accords with all the phenomena of the old time (9131, 3, 51, 63, 8, 9, 76, 94).

10867. *Olefiant gas.* The bubbles went outwards or diamagnetically in air (9161, 72).

10868. *Nitrous oxide.* The bubbles went outwards in air, according with former observations (9174).

10869. There would be no great difficulty in experimenting with bubbles of one gas in another—and, account being taken always of the constant action of the aqueous envellope, the gases might thus be compared one with another, as before by streams (9138, 45).

10870. I tried a tube with a larger end and larger bubbles, but did not find the effect more striking, for then one has to deal with more remote parts of the magnetic field, where of course the diamagnetic force is weaker.

10871. One can tell how a cylinder of one gas would set in an atmosphere of another gas at the same temperature and pressure by considering the effects and directions of streams, as in the old expts. (9138, 45, 54, etc.); and also how a cylinder of hot gas or a bubble would set or travel in an atmosphere of the same gas cold (9077, 9203).

10872. But if one could tell how a bubble of hot gas would move in an atmosphere of the same gas cold, but rarefied to the same degree as the hot, it would be important. Only I do not see how that is to be done. (p. 2192[1]).

10873. Or if one could tell how a bubble of rarefied nitrogen (or other gas) would set in dense nitrogen (or other gas) at the same temperature, that would be important; but cannot be done. (p. 2192[1]).

10874. The object is to eliminate in gases the effect of rarefaction from the effect of difference of temperature. It would tell much if we knew what was due to heat and what to attenuation.

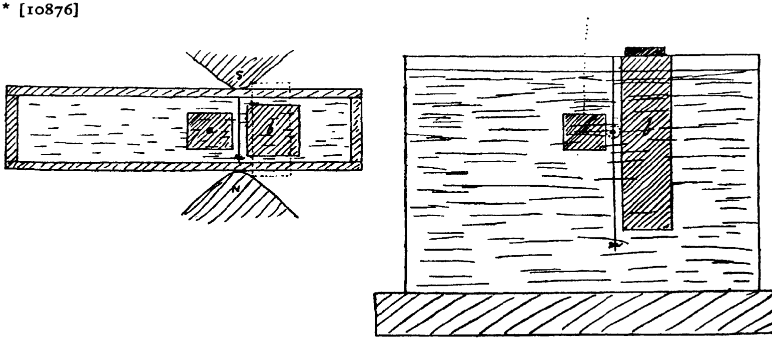
[1] I.e. par. 10897.

10875. A balloon went up on Saturday Evng. (22 instant) from Vauxhall. The evening was very clear and the Sun bright: the balloon was very high, so that I could not see the car from Queen Square, Bloomsbury, and looked like a golden ball. Ballast was thrown out two or three times and was probably sand; but the dust of it had this effect, that a stream of golden cloud seemed to descend from the balloon, shooting downwds. for a moment, and then remained apparently stationary, the balloon and it separating very slowly. It shews the wonderful manner in which [each] particle of this dusty cloud must have made its impression on the eye by the light reflected from it, and is a fine illustration of the combination of many effects, each utterly insensible alone, into one sum of fine effect. If a cloud of dusty matter, as powdered chalk or road dust, were purposely poured forth under these circumstances, it would give a fine effect both to those on earth and those in the balloon.

29TH JUNE 1850.

10876. In reference to the possible equatorial polarity (or equivalent effect) of diamagnetic bodies in the magnetic field, as referred to in 10816, 10817, etc. and the production of an extreme case, to discover if possible its existence, I suspended a cube of bismuth by a bifilar thread suspension 6 feet in length so that it could move in the equatorial direction always keeping parallel to itself. It hung in a rectangular glass trough filled with a solution nearly saturated of proto sulphate of iron, and another piece of bismuth, suspended on a wooden bar which rested on the upper edges of the trough, could be advanced to or from the hanging cube. The pointed poles of the great magnet (10856) were on the outside of the trough, the whole arrangement being represented by the plan and Elevation beneath*. The magnet was excited by 20 pair

* [10876]



of Grove's plates. The drawings are to full size[1]. The circle *o* (in elevation) is the place of the ends of the conical magnetic terminations or magnetic poles.

10877. Now here a diamagnetic substance in a magnetic medium was as highly contrasted as it well can be for the exaltation of the expected effects; and when the magnet was rendered active, the cube of bismuth *a* swung to the left, proceeding out equatorially half an inch nearly, and fell back into its place on taking off the power. The point was to know whether the distance to which it receeded was the same or greater or less when the second piece of bismuth *b* was brought near to or removed from it; i.e. whether there would be any effect of repulsion–or of attraction between the two bismuths in the equatorial direction.

10878. Therefore the piece *b* was removed back–the magnet made active–the cube *a* allowed to move out and take its place of rest; and then the piece *b* advanced into the magnetic field–but no change of place in *a* could be observed, either of further repulsion, as was expected, or of attraction.

10879. To prevent the motion of *b* disturbing the place of *a* through mechanical action on the intervening fluid, a screen of thin mica *m* was put across the fluid to a certain depth; but as this prevented the approach of bismuth *b* to the cube *a* after the latter had moved out, the place of suspension of the bifilar was moved to the right hand about half an inch or less, so that the cube *a* leaned against the mica when the magnetic force was not on, but when the magnetic force was on, was by its diamagnetic force just relieved and set free from it, remaining a little distance off. In this case the diamagnetic force and the force of gravity of the bismuth pendulum were opposed to each other, but it allowed the bismuth *b* to be brought much nearer to the cube *a*. Still no effect of repulsion or the reverse could be observed.

10880. Then took away the mica–in which case the piece *b* could be brought close to cube *a*; but wherever *b* was placed, it seemed to make no change in the final position of *a*. The place of *a* was well read off by placing a paper with marks on the top of the magnetic pole, as at , for the image of this paper reflected from the side of the glass cell and the image of the cube *a* view[ed]

[1] Reduced to $\frac{3}{5}$ scale.

directly were superposed, so that it was easy to see the image of the latter travel over the image of the former. The magnetic poles and the pieces of bismuth were varied in place so as to make the magnetic axis pass in various positions respective of them, but none of these changes evolved any effect of *b* over *a*.

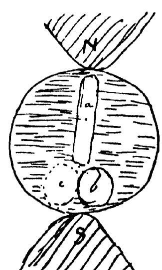
10881*. The cube *a* was turned angle-wise, i.e. 45°, with the idea that the differential effect of the sol. of Sul. Iron on the cube *a* might perhaps come out better thus; if, as was thought, any effect of separation of the bismuths might be due to the endeavour to increase the quantity of the better magnetic conductor offered by the iron solution between them—but still no effect was produced.

10882. So it does not seem as if, when the bismuth is driven off that the place of intense magnetic action may be occupied by the better magnetic conductor, the filling up of this place of intense action by bismuth throws any more power on to the mobile cube of bismuth *a*.

10883. If however we consider the enormous amount of magnetic power here produced, and its corresponding power of moving the cube *a* out scarcely half an inch—and then think of the portion of power expected theoretically between bismuth and bismuth, which must be only an infinitessimal proportion of the former—we can hardly expect that it should move the cube *a* through a sensible space; indeed there may well be a degree of equatorial polarity existing, yet quite unable to produce any sensible effect amongst these mighty forces.

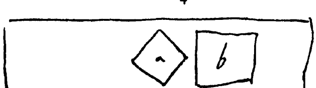
10884. Perhaps if a field of equal magnetic force were employed and the bismuth cube *a* were suspended by a bifilar torsion balance, or floated as a hydrometer—the effect of *b* might be eliminated (10894, 11060, 11104).

10885. Air in water is axial or apparently magnetic. Exp. Res. 2406, 2407, 2412, etc. Now being thus as a magnet, can it shew any thing like polarity, or is the degree of that force too weak to be made manifest in the midst of the strong forces concerned, otherwise than as the strong magnetic poles make it manifest by causing the weak poles of the air arrangement to point towards them? The figure[1] gives the plan of an experiment upon a scale of one half. A vessel of distilled water was placed between the

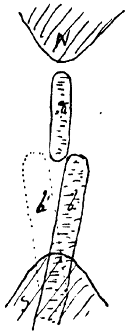


[1] Reduced to $\frac{3}{4}$ scale.

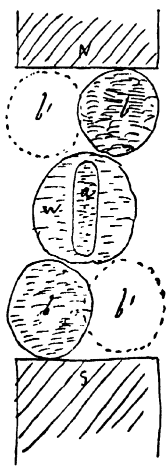
* [10881]



magnetic poles of the great magnet. A tube of air, *a*, of very thin glass was ballasted by a cube of copper and suspended by cocoon silk between the poles. It pointed well and easily, axially and magnetically, when the magnet was excited. A large thin tube (of air) was then held vertically in the water, first at *b* and then at *c*, to ascertain whether it caused any sensible deflection of the tube *a* (indicative of polarity); but no effect of the kind could be observed. I think there must have been a condition *equivalent* to polarity induced on the air in the tubes *a* and *b*; but that it was far too feeble to be sensible.



10886. Taking substances admitted to be magnetic, as solutions of Iron, Nickel and Cobalt, I tried whether one of these was sensibly polar in the Magnetic field. A solution of sulphate of cobalt in a thin glass tube *a* was suspended between the two poles and near to one of them; it pointed well when the magnetic force was on. Then a large tube of thin glass was filled with a saturated solution of Sulphate of iron and held against the other pole in the two positions indicated *b*, *b*; but no attraction between *a* and *b* could be observed in either position of it; i.e. no polarity either in the solution of cobalt or of iron was sensible. Yet they must have had some amount of Magnetic polarity, and far more in proportion than air could have in water—or bismuth in air.



10887. To make the polar effect more sensible, I placed the tube of cobalt solution in a diamagnetic medium much stronger than air, namely water, *w*. The pole ends were flat and in place of the tube of sulphate of Iron solution, two bottles of the solution were used. But when the power was on, whether the bottles stood in the places *b*, *b*, or in the opposed places *b'*, *b'*, the tube *a* preserved sensibly exactly the same position. I thought at times there were signs of a set more to the right or to the left, and it is just possible that visible indications were coming on, which more delicate arrangement might make sensible, but they must have been very small.

10888. Hence the expts. proposed, 10809, 10810, with air or a diamagnetic fluid in a more diamagnetic fluid (10885) would not give sensible results even though the terminations were so to say polar. And as solution of iron or cobalt in air shews no signs, so there is no reason to expect indications from the expts.

proposed, 10812, 3, 4, 5, though certain insensible effects may exist (10818, 9) (as regards Magnecrystallic polarity).

10889. In order to have an idea of the relative diamagnetic force of certain fluids which I wish to employ, a short cylinder of glass was suspended by cocoon silk successively in them. The fluids were Sulphuret of Carbon–Camphine–Olive oil–distilled water. The cylinder stood equatorially in the air and well so. It stood axially in all the fluids, taking up its place more or less freely, either as it was more or less axial in relation to the fluid, or as the fluid to be displaced was more or less mobile. It set least readily in the oil (olive) when cold. On making the oil hot so as to give it mobility, it set more readily than before, but not with the same directness as in the other fluids. In water it set as rapidly as in Sulphuret of Carbon or oil of turpentine. On the whole, I make the order thus

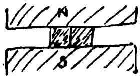
	Axial	
	Air	
	Glass	
in relation	Olive Oil	
to each other	Camphine	nearly equal
	Water	
	Sulphuret of Carbon	
	Equatorial	

10890. Does this distinction exist between Magnetic and diamagnetic bodies–that in a field of *equal force*, a long magnetic body will tend to set axially because of its facilitating so the concentration of force, whereas a long diamagnetic body will not tend to set equatorially as not by that helping concentration?

10891. But then in such a field, a weak *magnetic* body should not set equatorially in a stronger *magnetic* medium, and yet a long portion of the stronger medium ought to set axially in the weaker medium. Or a magnetic body should set axially in a medium weaker than itself, but not equatorially in a medium stronger than itself, in a magnetic field of equal force.

10892. This is not likely, for the action of two portions of magnetic matter used alternately as elongated portion and surrounding medium are almost certain to prove reciprocal. So that if the set of one be axial, the set of the other will be equatorial. Probably

the diamagnetic (10890) or the weaker (10891) will set equatorial because, if axial, it will tend to produce points of *dilution* of magnetic force, or the reverse of concentration; and the tendency to equalization of force (by their dissipation) will set the diamagnetic equatorial (10915, 6, 7).

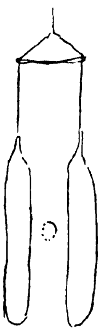


10893. Have not two pieces of iron, cubes or spheres, in a field of equal force, a tendency to separate equatorially, and is this not a species of equatorial polarity? Two pieces as in the figure do separate (10910, etc.).

10894. Examine this effect more carefully and then follow it out with weaker magnetic cubes and so on to diamagnetic cubes (10884).

10895. Reisch's[1] experiment of inverse polarity of bismuth. If true, then a tube of solution of sulphate of iron in a medium of stronger solution ought to shew this reverse polarity. Which is not at all likely, for very much stronger cases shew no polarity of the other kind. Exp. Researches 2690. M.S. notes, 10050, 406, 7, 691, 10830.

13 JULY 1850.



10896. May compare a rare and a dense gas in *thin* glass cylinders placed on opposite sides of the axial line in magnetic field as in the figure, the two under differential trial being suspended from one bar and that from a torsion or bifilar suspension above. May have tube[s] so like that they shall just counteract each other. May ascertain by trial of the tubes against a standard one that they do just compensate each other—or may record by the amount of torsion the diamagnetic value of the tubes used. Then, being sealed up when containing different gases or vacua, they may be compared against each other, and by noting the amount of torsion required to place the tubes equidistant from the axial line, their relative diamagnetic values may be absolutely compared with each other.

10897. So also a rare and a dense state of the same gas may be compared together and also a vacuum of one gas (so to say) with the vacuum or very rare state of another—and vacua with gases in various ways. Also hot and cold gas of equal density may be compared and so the effect of heat, if any, obtained (10872, 3, 4).

[1] For Reich's; Faraday's spelling of the name varies.

10898. In the same manner hot and cold copper, or hot and cold bismuth, may be compared (8433, 4, 5, 6, 503). Exp. Res. 2397.

10899. Also in reference to the question of conducting power, equal bulks of different metals may be subjected thus at once to the equatorial force and compared. The forms and conditions of the poles will require prior consideration. They may be thus, or thus; and also the mode of recording the places of the diamagnetic or other masses—which may be done upon a scale beneath or otherwise. The cylinders may be kept at a fixed distance by an interposed and separating guage*.

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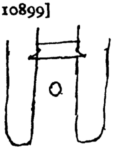
10900. A magnecrystallic body may be simply a conductor having different conducting powers in different directions, and at all events may, for the time, be so considered.

10901. Then its *conducting polarity* must be greater in the direction of the M.C. axis than in the transverse direction—and then ought it not to be more diamagnetic with M.C. axis transverse to axial line than when parallel to it? And may not this difference be enough to become visible by the differential method (10897), especially as one might turn the pieces on each side 90° round, so as to alternate the position of the M.C. axis.

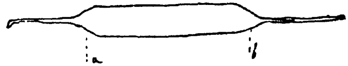
10902. I have selected a piece of very thin glass tube nearly equal in diameter; it is $\frac{3}{8}$ of an inch in external diameter and is green in colour as if it contained iron. I have prepared at the blow pipe two pairs of tubes from this piece having finally the form figured†. The two tubes of the same pair were made from parts of the tube contiguous to each other so as to have equality of bore, thickness, etc. Then one of these tubes was filled with oxygen and sealed up hermetically; the second tube of the same pair was also filled with oxygen, then exhausted by a good air pump and finally sealed up hermetically. So that the pair consisted of a tube containing dense oxygen and of another tube containing very rare oxygen—or an oxygen vacuum as it may provisionally be called.

10903. A second pair of tubes made from the same green glass was filled, one with dense nitrogen and the other had rare nitrogen or a nitrogen vacuum.

* [10899]



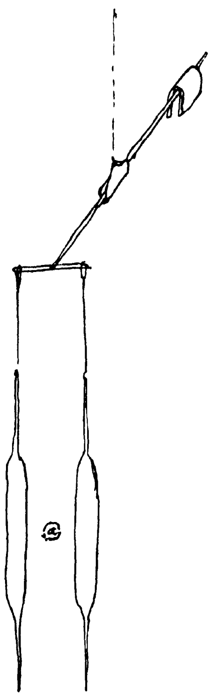
† [10902]



10904. Two pair of tubes were made in the same manner from a very thin piece of flint glass tube only $\frac{5}{16}$ of an inch in diameter and their contents made respectively dense and rare oxygen and nitrogen.

10905. The tubes were numbered by a little Brunswick black—and though the length of the cylindrical part, reckoning from *a* to *b* (10902), was the same for the tubes of each pair, it differed for the different pairs and is expressed below:

I.	Oxygen dense	Green glass tube, $\frac{3}{8}$ external diameter, $2\frac{1}{2}$ inches long.
II.	Oxygen exhausted	
III.	Nitrogen dense	Green glass tube, $\frac{3}{8}$ external diameter, 2 inches long.
IV.	Nitrogen exhausted	
V.	Oxygen dense	Flint glass tube $\frac{5}{16}$ external diameter, $1\frac{1}{2}$ inches long, glass between $\frac{1}{50}$ and $\frac{1}{60}$ of inch thick.
VI.	Oxygen exhausted	
VII.	Nitrogen dense	Flint glass tube $\frac{5}{16}$ external diameter, $1\frac{1}{2}$ inches long.
VIII.	Nitrogen exhausted	



10906. The further arrangement of these tubes is to be as follows. By a little sealing wax at one end, a silk thread is to be attached to each, which is to end in a loop, the threads being of such length that if all the tubes were hung up in a row, the *middle* of the length of each should be in the same line and about 3 inches from the top of the loop. Then a lever is to be prepared from a thin strip of clean deal having a cross bar at one end, and on to the cross bar the tubes are to be hung about an inch apart. The lever is to be suspended from a bundle of 10 or 20 cocoon threads by a card hook in which it is to lie, and either by shifting or by the counter-poise is to be retained horizontal. The bundle of silk fibres is to be made fast above to an index pen, so as to constitute a torsion balance which made [? may] be set and also have measurements of force made by the Index above. The place of the poles or rather of the Magnetic axis is indicated by the circle *a*. It is expected that at equal distances the glass tubes will equipoise each other and that the contents of the tube will tell against each other and make them take up a position which will shew the differential results; and this may be arranged either in air or in diamagnetic fluids (which will remove the power of the glass and make the action of the contents of the tubes a purer or larger result), or in

Magnetic fluids. In the latter case the magnetic condition of the fluid may probably be capable of accurate adjustment to the magnetic condition of the green glass, and so annihi[la]te the effects of the vessels altogether and leave the portions of gas to act purely against each other (10925, etc.).

10907. Theoretically it would seem that whether the tubes are suspended in a magnetic or a diamagnetic medium, the result ought to be the same. For their position is a differential result. In a magnetic medium, the stronger diamagnetic would go further off than the weaker; and in a diamagnetic medium the same thing would happen. So if the medium and the matter of the vessels were alike in magnetic or diamagnetic power, the result would be due entirely to the gases or vacua, etc.

10908. If one uses a glass a little less diamagnetic than water (10889) it would be easy to adjust the water exactly to it by a little solution of iron.

10909. Plücker objects that my media or [? are] not the same in power at different degrees of distance, because of the difference in the law of decrease of power by increase of difference. It is probable the difference is due to the polarity of the magnetic parts in his experiments, and that when the magnetic force is so small as to make this polarity insensible, so there is no effect of the kind he mentions, and no objection to a compound medium in that respect.

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10910. Working with the great horse shoe Electromagnet, ours, and 20 pair of Grove's plates. First for place of round balls of iron in field of equal magnetic force. Two pole ends of Iron from the Pharmaceutical Society, being $2\frac{1}{2} \times 1\frac{3}{4}$ square and 9 inches long (10761) were put face to face and prevented from coming in contact by 2 strips of wood; they were $\frac{7}{16}$ of an inch apart, so the Aerial Magnetic field was $2\frac{1}{2} \times 1\frac{3}{4}$ and only $\frac{7}{16}$ across. A little ball of iron was hung on a cocoon thread and being introduced into the space before the magnetic power was on, it was attracted to the one face or the other and adhered there. The attraction was considerable when on the middle of the iron face and required a good push with a slip of card to move it. Still, it was far less

than if it had been on the end of a pointed pole opposite a flat face or another pointed pole. The ball was $\frac{1}{15}$ of an inch in diameter. If the ball was midway between the two iron faces, then it remained there, and the attraction was not great when it was nearer to one side than the other until it was close to that side. The power increased rapidly very near to the iron (10884, 93, 4).

10911. The distance was now diminish[ed] to 0·3 of an inch; the same effects occurred but the attraction was much stronger.

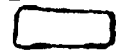
10912. Now made distance apart $\frac{1}{10}$ of an inch, so that the ball of iron occupied $\frac{2}{3}$rds of the distance, and then the effects far stronger. In both these latter cases it was easy to feel that as the ball was an imperfect sphere, so there were parts of its surface where it touch[ed] the iron planes by a larger surface or in two points at once; and it was very easy to feel that then the attraction was much stronger, as was to be expected.

10913. An oblong piece of iron of this size  was suspended by cocoon silk and suspended in the mag. field when about 0·4 wide and of equal force (10890), at least in the parts employed or very nearly so. As soon as the magnetic power was on, it set well and freely axially, and instantly went to one side or the other, clinging end on to the iron. If it was pushed over and laid sideways, it cleaved to the iron sideways because of the favourable contact and larger surface of the iron pole [illegible] and relieved by it; but if raised end up, when once so it remained so and cleaved to the iron with great power. If pushed along by a card it quivered with vibratory energy, shewing how powerful the force retaining it in this position. The advantage now was that it could discharge further across the ærial magnetic field though in less contact with the iron.

10914. Hence iron points axially in a field of equal power. A piece of soft iron therefore, suspended by its center of gravity, ought to point by magnetic lines of the earth; but the power is here probably far too feeble unless the iron be assumed as of great length.

10915. *Tin.* Employed a piece of tin about this size  as a diamagnetic, non Magnecrystallic body, and suspended it in the field of equal forces (10891, etc.). It set axially at once and I have no doubt from iron present; but as it may be supposed to be a body

diamagnetic with one force and magnetic with another, compare it with results of experiments to be made hereafter on that point.

10916. *Phosphorous.* Used phosphorous as a pure diamagnetic, and when a cylinder of this size  was suspended by cocoon silk, one fibre, in the equal force field just wide enough to allow it free room, it seemed to be perfectly indifferent in position (10892).

10917. Then to exalt the conditions as far as possible I made the surrounding medium a magnetic body i.e. saturated solution of Sulphate of Iron in a flat glass cell. The faces of the Magnetic field were now made by using two flat plates of iron $7\times 2\frac{1}{2}$ inches and 1 inch thick, and connecting them by the two iron bars before spoken of with the poles (10910). When the cell was in its place the mag. field surfaces were therefore $7\times 2\frac{1}{2}$ and $1\frac{3}{8}$ inches apart. Now the middle of this magnetic field must have lines of very nearly equal intensity, but the phosphorous in it set clearly equatorially—and constantly so.

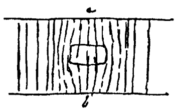
10918. When the iron solution was removed and every thing else, as to distance, etc. remained the same, air being now in the field, the phosphorous was quite indifferent.

10919. Hence I conclude that a diamagnetic surrounded by a medium either magnetic or less diamagnetic than itself in a field of equal force tends to set equatorially in length, and that a magnetic surrounded by a medium either diamagnetic or less magnetic than itself tends to set axially; but that where the differences between the setting body and the medium are small, this becomes insensible, as in the case of phosphorous and air.

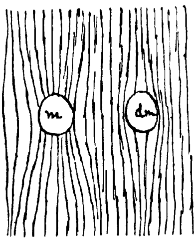
10920. In all the foregoing cases, however, the field only remains of equal force when the setting body is *not in it*; for when in it, the force is diminished or increased as the body can worse or better carry on these lines of force. A piece of iron, as the ball or oblong spheroid (10910, 3), causes great elevation of the force there, and also in the parts between it and the iron faces, and also on the parts of the iron faces opposite to it; for the equal distribution of the power by the iron face is disturbed by its presence. The reverse must happen with a diamagnetic, if it be as I believe a bad conductor, i.e. worse than air or a vacuum (   ). So a field of equal force is no longer a field of equal force if a magnetic body be in it; and so also in the expt. of the phos. and Sul. Iron

solution (10917), the force is doubtless weakened in the phosphorous and in its neighbourhood.

10921. MEM. The lines which cross a field of force must always be equal in power in any section through the whole of them. A section taken through the phosphorous must have as many (units of power) as a section taken through the fluid between it and the iron face of the field. Hence if there be fewer going through the phosphorous, they must find their equilibrium through the adjacent fluid by a little concentration at the ends of the phosphorous and also a little correspondent diminution or weakening of them at *a* and *b*, if the iron faces be within a certain distance.



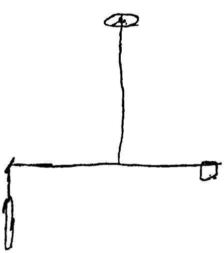
10922. In a field of equal power and unlimited, as that of the earth's influence, a magnetic body should gather up on to itself and pass forwd. more of the lines of force than those corresponding to its projection or cross area; and a diamagnetic body should do the contrary, as in the figures. This case is in fact the same as the former, i.e. it passes into it. Only as the iron is a good conductor or is equivalent to one, it allows of the accumulation of the lines of force more easily than a worse conductor as air or vacua.



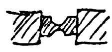
10923. *Change of volume.* A two ounce bottle was filled with a solution (saturated) of proto sulphate of iron and one of the fine guages (10715) attached to it. It was then placed in a wooden block between the two poles (10910) as near as they could be brought leaving the bottle free and without pressure; and then the full magnetic power put on and off alternately—but not the smallest change of volume of the Solution occurred.

10924. As to iron being a mere conductor or assuming the state which a magnet has when it is permanently magnetic and contains within itself the source of its activity. When the Magnet is made with the poles all touching, so that the iron is a complete circuit and has body throughout—then throwing off the current does not undo the condition of the iron, for it remains powerfully magnetic until contact is broken. Hence probably whenever iron is in the magnetic field, it becomes polar as a magnet is polar, and may not be considered as a mere conductor without polarity (   ).

10925. *Gases differentially compared.* I have made a torsion balance and arranged all things as proposed (10906). At present the suspension is by a bundle of 60 cocoon threads—the cross bar



has a length of 8$\frac{1}{2}$ inches from point of rotation to the gas tube suspension. Used the double cone core between the irons from the Pharmaceutical Society (10910). All answered admirably.



10926. Hung up I and II (10905), i.e. dense and rare oxygen in green Glass tubes. Found that either was attracted and that whichever had the advantage went up close to the axial part of the core. This was due to the magnetic condition of the glass. These tubes therefore can only be used in a magnetic medium having equal power with the glass, and that can easily be prepared.

10927. *Oxygen dense and very rare*, i.e. V and VI (10905), being in the smaller flint glass tubes, were hung up. The moment the magnetic power was on, the differential effect was excellent. The dense oxygen went up towards the axis and the rare oxygen, almost a vacuum, went out on the opposite side. Yet the dense Ox. did not go quite up into contact with iron at axis, but remained out about $\frac{1}{8}$ or $\frac{1}{7}$ of an inch, whilst the other was full $\frac{4}{8}$ distant from the other side. If the dense oxygen were brought into contact, it went off again until the two tubes had taken their place of rest, on opposite sides of which place of rest the torsion beam vibrated rather quickly, shewing the power which the portions of Gas and the tubes had (10974[1]).

10928. I changed the tubes to contrary sides; still the same effect was produced. The dense oxygen went in and the rare oxygen went out.

10929. The effect must be as follows. The tubes are diamagnetic, hence both would go outwards, and as the force increases more rapidly inwards than as the distance, so if one were nearer than the other it would go out until the tubes, being of equal force, were equidistant. The equidistant would be the stable position. Then if the contents differed and were not equal when equidistant, that which tended most inward or least outward would go inwards until it[s] tendency was counterbalanced by the diamagnetic glass which it would carry in with it. And so it clearly appears that Dense Oxygen tends to go to the axis far more powerfully than the oxygen vacuum, and would doubtless go up to it but for the diamagnetic force of the glass keeping it off. The latter indeed is a measure of the former.

[1] ? 10970.

10929 *a*. By immersing the tubes in media of equal diamagnetic or magnetic tendency with themselves, their effect would be removed and the portions of gass alone be influential against each other.

10929 *b*. So dense oxygen is more magnetic or less diamagnetic than rare oxygen.

10929 *c*. Does it not follow that if the tubes were perfectly compensated and then gases subjected to action, a magnetic gas would not preserve its equilibrium but the bubble which is nearest would continue to approach the magnetic axis until up to it (10965); whereas a diamagnetic gas would preserve an equal distance on each side, being then in stable equilibrium? Yes, if in Vacuo, but not if in other media. For if both bubbles were magnetic, but the Media more magnetic, both would remain out equidistant as diamagnetic bodies.

10929 *d*. If one were more magnetic than the other, then the difference would appear, but not so strongly in a medium stronger than both as in a medium only equal to the strongest; because in the first case neither could go up to the axis, and in the second, one could. If therefore the medium were less magnetic than either or diamagnetic, then the effect would be greater.

10929 *e*. Consider these relation[s] on presently, in reference to diamagnetic bodies expanded, etc. in different Media (10964).

10929 *f*. *Nitrogen dense and very rare*, i.e. tubes VII and VIII (10905) in flint glass—were hung up in place. Both tubes opened out as if both diamagnetic, but as far as I can judge at present, are equidistant from the axial line. On every time of repeating the experiment, the same effect occurred. If the lever was moved until the distances of the two were different; then, when left at liberty, the whole system vibrated to and fro about the equidistant position, shewing that that was a fixed and stable position. Hence it would appear that dense and rare Nitrogen are alike and that in this respect Oxygen and Nitrogen differ in a very extraordinary manner.

10929 *g*. It may be and probably is so that a Vacuum is the true Zero, and that Nitrogen is close to it. Then rarefying it would make little or no difference.

10929 *h*. Two bubbles of air (glass compensated) alike in every

respect, can only take the stable position (10929) if in a medium a little more magnetic or a little less diamagnetic than themselves. Hence a means of experimenting in cases where the differences are small, for by making the medium the same or even a little on the further side of the gas to be examined, differences will appear which would not appear without. Thus though rare and dense nitrogen appear the same in Air (Nitrogen + Oxygen), they may not appear the same in pure Nitrogen.

10929 *i.* Though, if rarefaction produces a difference, it will tell in the *same direction* in all media, it will not tell with the *same strength* in all (10966).

10929 *k.* *Dense Oxygen* and *dense Nitrogen*, i.e. V and VII (10905), were put up at once. Oxygen went inwards strongly and Nitrogen outward, in about the proportions of dense and rare oxygen. The sides of the tubes were changed, with the same result. So Oxygen is well Magnetic and Nitrogen well diamagnetic to each other—at this same pressure of 1 atmosphere (10932).

10929 *l.* *Rare Oxygen* and *Rare Nitrogen*, i.e. VI and VIII (10905). As nearly alike as possible. Changed sides with the same result. Hence the Vacua of Oxygen and Nitrogen (   ) are nearly if not quite alike.

10929 *m.* Further, these Vacua are the same nearly as Nitrogen of 1 atmosphere.

10930. *Dense Oxygen* and *Rare Nitrogen*, i.e. V and VIII. The dense oxygen in very strongly—the rare nitrogen out.

10931. *Rare Oxygen* and *dense Nitrogen*, i.e. VI and VII (10905). Like each other.

10932. *Dense Oxygen* and *dense Nitrogen*, i.e. V and VI[1] (10905) again. As before, the oxygen powerfully in and the Nitrogen out (10929 *k*).

10933. So the Vacua of Oxygen and of Nitrogen and dense Nitrogen apparently alike—but oxygen of 1 atmosphere very magnetic to all of them.

10934. In reference to Gravity (10087). Supposed that the assumed currents have relation to the line of approach (joining the gravitating bodies) and not to space generally. Hence that 2 bodies

[1] This should be VII.

approaching or receeding would have opposite directior space generally, though the same as to the line of approac
10935. Else two bodies approaching would have opposi in the surfaces towards each other, and tend by their a merely to cause a force of attraction—which would lead tc sistant consequences, for then a body between 2 others w in instable equilibrium, for the least approach towards one make it continue to approach that one and receede from th

10936. Oxygen would appear to be Magnetic—for whe it is more axial than when rare. Nitrogen may not be diam for when dense it is not more diamagnetic than when rar there are probably other gases which, being diamag Nitrogen, may lose of their diamagnetic force by rarefacti
10937. In old experiments (Exp. Researches, 2408), bism diamagnetic in Vacuo and so there appear to be bodies w really diamagnetic. Still, as Air cannot be at Zero becaus Oxygen in it, so we must have a new arrangement of M and diamagnetic bodies, for the former arrangement had re to experiments made in *Air*; and Platinum, Arsenic, et change sides, in a true Vacuum or Zero. *
10938. Must ascertain the effect of temperature—how it Oxygen or nitrogen. Perhaps a moderate difference ma oxygen. Then there will be some effect in nature at the and the equatorial parts of the globe—a difference of con by a difference of temperature may have something to c Magnetic storms.
10939. Perhaps weaker magnetic powers may act on gase than on dense magnetic or diamagnetic bodies as bismuth, A weaker, i.e. a permanent, magnet may affect gases.
10940. May perhaps make a permanent instrument, a sp magnetometer.
10941. The weight of a bubble of oxygen is very small would its weight of iron, in a sphere placed at the same equ distance from the axial line, do in comparison of it? Wei weight, oxygen may be very magnetic. But then it is a ga
10942. What will cooling do with it? Or heating?
10943. The Magnetic condition or relation of air ma

continually and be an important element of meteorology. It would be good to have an indicating instrument—constant and true in its action—so as to bear observation from day to day.

10944. A Glass torsion thread, a short lever—a reflector on the lever under the thread—a constant Magnet. Observe by a telescope—the coincidence of a scale and its reflexion in the mirror.

10945. Might even tell the composition of the air by such an instrument, *as to the oxygen*—having only one diamagnetic body hung up, i.e. if nitrogen invariable. Or if that variable, then tell some other fact of greater importance.

10946. The atmosphere in small portions only is like a field of equal magnetic force—in larger portions becomes a differential field, increasing or diminishing in power from one part to another.

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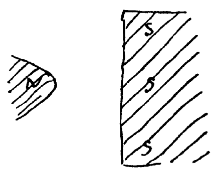
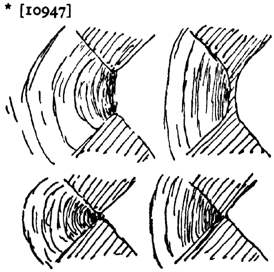
10947*. As to the form of the core or of the poles of the magnet for differential arrangements. When the opposed bodies are on opposite sides of the axis (Magnetic), then the figured forms would give fields of force in which the lines of magnetic power would vary in difference of proportion and also of form very considerably.

10948. Another form of poles for the production of differential effects might perhaps be conveniently made in which the bodies experimented with might go from stronger to weaker places along the line of magnetic force, as thus†; where N may either be soft iron or a permanent steel pole of a permanent magnet and S S S a mass of soft iron opposed to it and connected with the South pole of the same magnet.

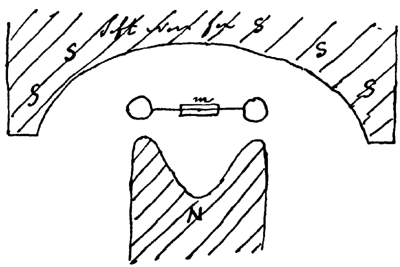
10949. It is like enough that if S S S were below the level of N, a difference would occur; but on the whole, it would seem better to make it rise and fall above N, so as to obtain at the points of N the greatest concentration of force and from thence outwards the most rapid diminution.



* [10947]



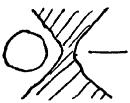
† [10948]



Could try the effect of Reisch's piece of iron in different positions, as long as it increased the sensibility of the diamagnetic bodies.

The mirror *m* (10948) might be a concave mirror inclined upwards*, so as to send the image of a flame to a focus above on a screen on the wall, where it will travel. Would be good for the Lecture room.

10950. The weight of a cube of oxygen half an inch wide is not quite ·06 of a grain. If a bubble of oxygen containing the eighth of a cubic inch were on one side of the axial line and a little sphere of iron weighing ·06 of a grain were on the other side, the centers of the two spheres being equidistant, which would go inwards? I suppose the iron, but am not quite sure; for oxygen may come out by this sort of comparison very magnetic.



10951. Perhaps the iron (or nickel or cobalt if they are used) ought to be a wire as long as the diameter of the bubble, and placed as horizontal radius to the Magnetic axis on the opposite side to the bubble. If iron too strong, could either shorten its length or, by approaching the oxygen, gain the position of instable equilibrium.

10952. But in estimating the Magnetic powers of bodies as conductors or perhaps otherwise, we should probably consider them as of equal bulks, not as of equal weights. When considered as sources of inherent power, or as if each molecule had its own store of force, then weights may come in.

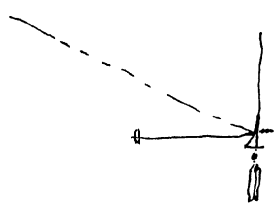
10953. In an ærial Magnetic field, as that of the Earth's atmosphere, bulks would seem to have more importance than weights, especially with bodies considered simply as conductors.

10954. Must try whether the difference between O. and N. or any other two bodies is proportionate, i.e. whether it is the same for a current of 5, 10 or 20 pr. of plates. Perhaps no change of place but only the assumption of that place more readily and with more force as the Magnetic power is greater.

10955. Must not only ascertain the effect of warmth or heat on gases—but also on *vacua or space*. That IMPORTANT.

10956. The changes of temperature in air or the parts of air, or

* [10949]



the changes of place of warm and cold air, may easily cause changes in the direction of the lines of force of the Earth—strengthening in one place and weakening them in another, and so causing magnetic storms.

10957. May perhaps here establish a new relation of Terrestrial Magnetism—and develope a new element of force to be introduced into the study of that subject. Perhaps the causes of many of the variations we have recognised may exist in the atmosphere.

10958. Perhaps the cause here of the daily variation and even of the larger annual variations.

10959. May have been on the wrong look out in searching for these *within* the earth. *All* may be in the *air* and in *space*.

10960. These Magnetic variations may be a great function of the oxygen of the air.

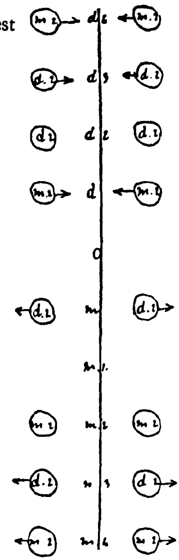
10961. If these fancies well founded, then indeed the Magnetic condition of the Air most important, as originally supposed (Exp. Res., 2440, 2442. Also end of letter on flame and gases). The earth *may be* in respect of its magnetic state perfectly stable, or change only as it is affected by these changes.

10962. It is not unlikely that in all that is related to the annual or daily variation or changes of seasons even in long terms, i.e. to all that is external to the Planet, the variations of its magnetism may depend upon the magnetic affections of its atmosphere.

10963. If change of temperature alters the conducting power of air for magnetism, that may cause motion and so be a source of current in the air and even of storms.

10964. Will not the following views and figures represent properly the relations of two bodies to be subjected at once to a magnetic field having axial power, according as the medium they are in is more or less magnetic? To represent different degrees of magnetism, I will use *m*, *m*2, *m*3, *m*4, and for diamagnetism *d*, *d*2, *d*3, *d*4, etc. So the central line represents the medium gradually increasing in diamagnetic power upwards and in magnetic power downwards (10919*e*[1]).

[1] ? 10929 *e*.



✓ i. Unstable equilibrium—the nearest will go inwards

1. Unstable—the nearest will go inwards

ii. Equilibrium any where indifferent—stand at any distance

✓ 2. Unstable—the nearest will go inwards—not so strongly as in i

iii. Stable equilibrium: if disturbed will resume this position

✓ 3. Equilibrium any where indifferent—stand at any distance

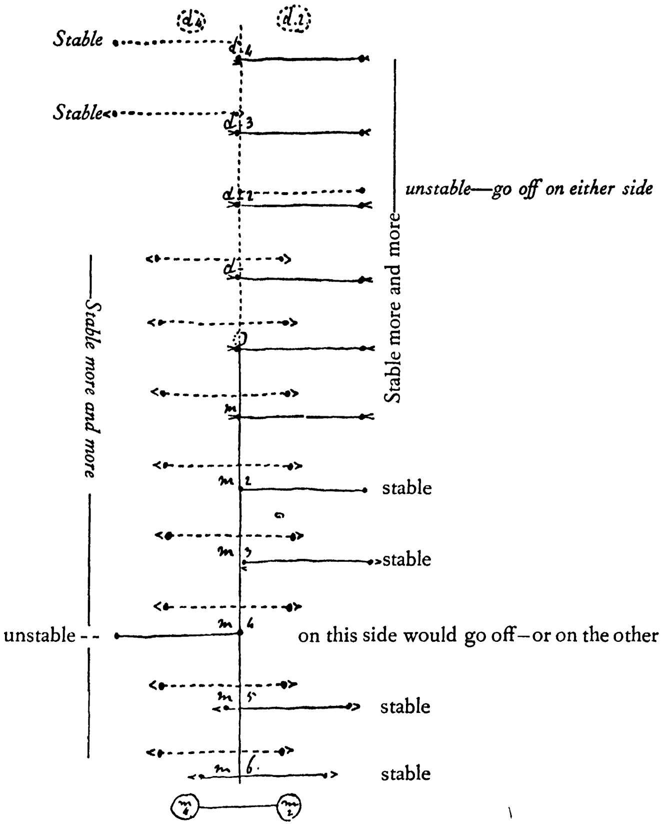
iv. Stable—and more so than iii

✓ 4. Stable—if disturbed will resume this position

10965. So two equi-bodies in media more magnetic or less diamagnetic take a stable position at equi distances, provided one is on each side of the axial line; if both are on the same side, then both are driven off. Two equi-bodies in media less magnetic or more diamagnetic, if placed equi distant and one on each side, are then in a position of *instable* equilibrium, and if one be made nearer than the other, it will continue to go inwards till up to the axis or very nearly so. The stability or instability increases with the difference between the bodies and the media.

10966. The following scheme represents I think the positions which would be taken up by two connected bodies of unequal magnetic or diamagnetic forces, if placed at the beginning equidistant from the axial line and one on each side. The pencil or red represents the diamagnetic substances and the black the magnetic substances. The original differences are taken as constant, i.e. as $\frac{m}{4}$ and $\frac{2m}{8}$, and also $\frac{d}{4}$ and $\frac{d}{2}$; the dots on the line

representing medium, i.e. the two dots connected across it represents the place of these connected bodies[1].



10967. I want a name in opposition to dia-magnetic and as a subdivision of Magnetic, which may apply to Iron, Nickel, Cobalt

[1] The parts of the diagram shown dotted, and the words in italics, are in red in the original.

and their magnetic compounds. Will Terro-magnetic do? Terro from the earth as a standard. Then

Magnetic will subdivide into {
Terro magnetic (bodies) as Iron and Oxygen, etc.
Diamagnetic (bodies) as bismuth, Phosphorus, etc.

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10968*†. I have had some flint glass bulbs blown about the size and shape shewn[1], very thin both in the wide and the narrow part. They were intended to be nearly alike and are not very much different; still the diameter and length of the wide part and the weight do differ considerably. These were filled with various gases at different pressures, being drawn out, as represented, with fine passages at *a* and *b*, then fitted into a cap, attached to a transfer jar and gas sent through until the contents were certainly good. Then the end *b* was sealed at the lamp, and afterwards *a* drawn out and sealed; or if $\frac{1}{2}$ or $\frac{1}{3}$ or vacuum atmosphere was to be in the tube, the vessel was attached to the air pump and proportionally exhausted and then sealed at *a*. All this is easily done with most gases. The diameter of the finished vessels was about $\frac{5}{8}$ of an inch and the barrel part an inch or less in length.

10969. The following set were prepared and numbered:

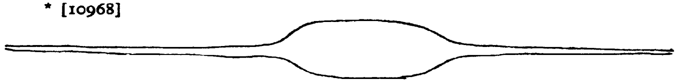
1. Oxygen 1 *a*.
2. Oxy. $\frac{1}{2}a$.
3. Oxy. $\frac{1}{3}a$.
4. Oxy. Air pump vacuum.
5. Nitrogen 1 *a*.
6. Nit. $\frac{1}{2}a$.
7. Nit. Air pump vacuum.
8. Hydrogen 1 *a*.
9. Hy. $\frac{1}{2}a$.
10. Hy. Air pump Vacuum.
11. Carb. Acid 1 *a*.
12. Carb. Acid $\frac{1}{2}a$.
13. Carb. A. Air pump Vacuum.
14. Carb. ox. 1 *a*.
15. Carb. ox. Air pump Vacuum.
16. Oleft. Gas 1 *a*.
17. Oleft Gas. Air pump Vacuum.
18. Nitrous oxide 1 *a*.
19. Nitrous oxide V. air pump.
20. Nitric oxide 1 *a*.
21. Nitric oxide V. air pump.

(11023 for further list)

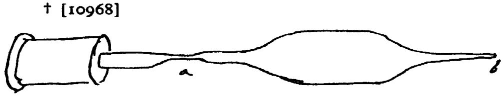
Barometer was 29·75. No. 21 became very pale yellow whilst sealing at *a* from leakage of a stop cock and when sealed was tight. After the following experiments, I opened it under water,

[1] Reduced to $\frac{3}{4}$ scale.

* [10968]



† [10968]



when a small bubble of gas about the size of a large pea was left. So it had leaked a little by the plug of the stop cock after taking it off from the pump, but the sealing was perfect. Though not a vacuum, it approached towds. it, for I do not suppose there was $\frac{1}{20}$ of an atmosphere within.

10970. The *Oxygen* 1 *a.* and *Oxygen* V., i.e. 1 and 4 (10969) were hung up as on the former occasion (10906, 10927) and acted beautifully; the dense oxygen went strongly inwards and the oxygen vacuum outwards as before, and that on whichever sides the tubes were hung. The suspending bundle consists of 60 silk cocoon threads, but an amount of torsion equal to ten revolutions of the index, or $10 \times 360°$, set out the dense oxygen very little and was not at all sufficient to bring the two tubes to equidistances. I should think 30 or 40 revolutions would not do it. Hence great power of set, according with the quickness of vibration before referred to (11040).

10971. In reference to any thing like accurate results, must remember hereafter that the bulbs are unequal in size and weight, but these for oxygen were selected as nearly alike as might be.

10972. Then compared oxygen of unequal and known densities, thus:

Oxy. 1 *a.* and *Oxy.* $\frac{1}{2}$*a.* The 1 *a.* goes well in and the $\frac{1}{2}$*a.* is forced out.
Oxy. $\frac{1}{2}$*a.* and *Oxy.* V. The $\frac{1}{2}$*a.* well in and the *Vacuum* out.
Oxy. $\frac{1}{2}$*a.* and *Oxy.* $\frac{1}{3}$*a.* The $\frac{1}{2}$*a.* well in and the $\frac{1}{3}$*a.* out.
Oxy. $\frac{1}{3}$*a.* and *Oxy.* V. The $\frac{1}{3}$*a.* now in and the *Vacuum* out.

10973. The results were beautiful, shewing that as the oxygen was rarefied it became less and less powerful in its tendency inwards, up to a Vacuum. Hence can hardly resist the conclusion that it is a magnetic body and powerfully magnetic, and that as it is rarefied it loses of this power. The loss seemed to me very like as if in proportion to the removal of oxygen.

10974. *Nitrogen* 1 *a.* and *Nit.* V., or 5 and 7 (10969). Were equidistant; I could not tell that one was nearer than the other. In vibrating also they vibrated about the middle distance, but the Vibrations were quick as before (10927). *N.* 1 *a.* and *N.* $\frac{1}{2}$*a.* were also alike. Also *N.* $\frac{1}{2}$*a.* and *N. V.* were alike. So that if a vacuum be taken as Zero and the Vacua of *Ox.* and *N.* are sensibly

alike, then N. up to 1 atmosphere is the same as Zero, i.e. Nitrogen is neither a magnetic nor a diamagnetic body.

10975. *Hydrogen* 1 *a.* and *Hy.* ½*a.*; also *Hy.* 1 *a.* and *Hy. V.* (10969). I thought occasionally that *Hy.* 1 *a.* tends most strongly *to go in*, but the results were so very nearly alike that I could not say so without a more delicate arrangement. Hydrogen must be near Zero.

10976. *Carbonic acid gas* 1 *a.* and ½*a.*; also 1 *a.* and *V.* (10969). The ½*a.* seemed a trace in–but the other case as nearly as possible alike. I must remember in these cases the possible differences of the *glass tubes* themselves. Hydrogen must be near Zero.

10977. *Carbonic oxide* 1 *a.* and *V.* I think the V. goes in towards the axis as if the 1 *a.* and therefore the Carbonic oxide were diamagnetic by comparison.

10978. *Olefiant gas* 1 *a.* and *V.* The V. goes inwds. and the 1 *a.* outwards–I think clearly so–and though the effect *may* be due to difference of the tubes, still I think it is rather due to the Gas, and that this is a case of a true diamagnetic body, i.e. of a body on the opposite side of Zero or a vacuum to what oxygen is.

10979. *Nitrous oxide* 1 *a.* and *V.* Sensibly alike.

10980. *Nitric oxide* 1 *a.* and *V.* The 1 *a.* goes in most but nothing like oxygen. Still, this gas appears to be truly magnetic. The vacuum as before said (10969) was coloured, but being opened was found very good for a first experiment.

10981. The order seems to be

Oxygen–Nitric oxide–Nitrogen–Carbonic oxide–Olefiant Gas
Hydrogen
Carb. Acid
Ns. Oxide

10982. Now compared single atmospheres of the different gases–all being at one pressure, i.e. 29·75 inches Barometer.

Oxy. and *Nitrogen.* Oxygen strongly in.

Oxy. and *Hydrogen.* Oxy. in, but I think not so strongly as with Nitrogen.

Oxy. and *C.A.* Oxygen well in; more than with hydrogen.

Oxy. and *C.Ox.* Do. . . . . Do. . . . . . as C.A.

Oxy. and *Oleft. Gas.* Oxygen well and strongly in.

Oxy. and *Nitrous Gas.* Oxygen well in.

Oxy. and *Nitric ox.* Oxygen in, but not so strongly as in the last case.

The order roughly of these would be Oxygen–Nitric oxide –Nitrous oxide–Hydrogen–Nitrogen–Carbonic acid–Carbonic oxide–Olefiant gas.

10983. *Nitrogen* and *Hydrogen.* The hydrogen in, the Nitrogen out.

Nit. and *C. Acid.* Very like each other.

Nitrogen and *Carb. oxide.* I think the C. ox. in and the Nitrogen out, but must not forget possible differences of glass.

Nit. and *Oleft. Gas.* Nitrogen out–Olefiant gas insensibly.

Nit. and *Ns. oxide.* Nitrogen out. Ns. oxide in, clearly.

Nit. and *Nc. oxide.* Nitrogen out. Nc. oxide in, well.

The order here apparently Nitric oxide–Nitrous oxide–Hydrogen–Carbonic oxide–Oleft. Gas–Nitrogen–C. Acid.

10984. *Hydrogen* and *C. Acid.*–very like.

Hy. and *C. oxide*–very like.

Hy. and *Oleft. gas*–very like.

Hy. and *Nitrous oxide*–The hydrogen out and the Nitrous oxide in as if it were magnetic. I think the effect is clear. Still, there is not much difference. Again–are very like.

Hy. and *Nitric oxide.* Hydrogen out. Nitric oxide in.

The order here apparently *Nitric oxide–Nitrous oxide* and then Hydrogen, Carbonic acid, Carbonic oxide and Oleft. gas nearly alike.

10985. *Carbonic A.* and *C. ox.* Very like.

C. Acid and *Oleft. Gas.* I think the C. A. out and the Oleft. gas in a little.

C. acid and *Nitrous ox.* C. A. out. Ns. oxide in a little.

C. acid and *Nitric oxide.* C. A. out and Nc. oxide well in.

The order Nitric oxide–Nitrous oxide–Oleft. gas–C. acid–C. oxide.

10986. *Carb. oxide* and *Oleft. gas.* Very little difference.

Carb. oxide and *Ns. oxide.* C. ox. out. Ns. oxide in a little.

Carb. oxide and *Nc. oxide.* C. ox. out. Nitric oxide well in.

Order–Nitric oxide–Nitrous oxide–C. ox.–Olefiant gas.

10987. *Olefiant gas* and *Nitrous oxide.* Oleft. out. Ns. oxide in.

Oleft. and *Nitric oxide.* Oleft. out and Nc. well in.

Nitrous oxide and *Nitric ox.* Nc. oxide probably a little in.
Order–Nc. oxide–Nitrous oxide–Oleft. gas.

10988. I then compared different Vacua together, i.e. vessels which, having been filled with the particular gases, had then been exhausted by a good air pump and hermetically sealed. The oxygen V. was put in the torsion balance against the others, namely the *Hy. V.–N. V.–C. acid V.–Oleft. V.*–and *Nitrous Oxide V.* They were all sensibly alike, except that when the *Ox. V.* and the *C. A. V.* were opposed, the Oxy. V. perhaps went in a little.

10989. So allowing for the irregularities of tubes, the *Vacua* appear to be very nearly alike.

10990. As to the place of the differential bodies with Strong and weak Magnetic power, I put up *Ox.* 1*a.* and *Ox. V.*, which differ very much in place, and then used the Magnet urged by the 20 pr. of Grove's Plates, and noted the places of the tubes as well as I could. Then I urged the magnet by only 5 pr. of plates–the place of rest or of stable equilibrium was sensibly the same. Used only 2 pr. of plates: still the place of rest the same. The tubes assumed and retained this place with far less power with the smaller number of plates than with the larger. Hence difference of magnetic force will not change the indication of the proposed instrument.

10991. I must have *a good and perfect instrument* made with carrying stages, tables, etc.; observing by a reflector on the beam and the use of a torsion thread, and covering all if needful by a glass shade.

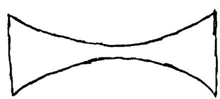
10992. The *torsion axle* ought to move in the center of a graduated plate moving on a stage; or the axle carrying the thread ought to move in a collar carrying the pin–the graduated circle being fixed. So that in any case the pin may be set to 0° and then the torsion adjusted without disturbing it, and finally both axle and pin move together.

10993. The *torsion thread* had probably best be of Glass. I do not see that differences in its thickness or rigidity or torsion will introduce *any alteration* into the expression of two observed numbers, because their proportion will remain the same what[ever] change may be made in this suspending film.

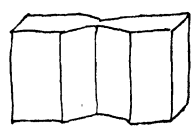
10994. The Magnet may vary much *without any change* in the measured result (10990). As it is stronger or weaker, the measured forces are stronger and weaker, but apparently always in the same proportion. From the great power exerted (10970), I have no doubt a *constant permanent* magnet may be used, but I do not see any advantage in that except that the battery can be dispensed with and the magnet be always ready. In that case there must be a power by rack work of raising and lowering the torsion lever and suspended bodies into and out of place for experiment and adjustment of Zero, and indeed in all cases that will be desirable.

10995. A moderate Electromagnet will probably do–excited by one or two pairs of plates in brine or weak acid. The magnet may easily be a special one and made part of the instrument.

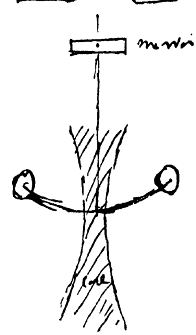
10996. The *core* for the Magnetic field had better perhaps be more of this form–for then it will give lines of force diminishing less rapidly in power, and so make a longer journey for the two opposed bodies, and they will be more nearly in the equatorial plane. I do not see that differences of core will at all affect the measurements of the power, provided the cores are true and symmetrical in shape, but by variation of them great differences in the degrees and delicacy of indication may be obtained.



10997. Perhaps a core of this form might be good, reducing the middle very much. Probably, however, not so good as a core which has a figure of revolution, unless indeed a slower decrease of power be required.



10998. If the lever arm of the torsion balance be made short, for instance from $\frac{1}{2}$ an inch to 2 inches, and the cross bar at the end be from an inch to 3 inches or more, then a good rotation effect will be obtained round the axis of revolution; i.e. the torsion thread and a mirror placed there will revolve a reflected ray so as to have a very sensible indication indeed. At the same time, increasing the length of the cross bar holding the two objects will make the visible motion much more sensible to the eye and give a great power of increasing or modifying the sensibility or steadiness of the instrument.



10999. If the reflector be flat or concave or convex, we get so a method of adjusting the rate of delicacy or variation of place

of the zero point to any required degree. Concave will give a greater and convex a lesser displacement than the flat mirror.

11000. The medium which occupies the Magnetic field and which is displaced by either of the moving bodies must be more near to *M* than to *D* (these representing the extremes of magnetism and diamagnetism) than either of the bodies to be tried, or them and their accompanying envellopes, or else they will have no place of stable equilibrium. But if it have this necessary superabundance of M. force, then it does not appear that, whether it have more or less, or whether it be liquid or gaseous, that will make any difference in the measurement; for though the two bodies are directly related to the medium, they are through it finally related to each other, and when they are by the torsion brought back to the equidistant position, that relation remains the same in expression (10964, 5, 6).

11001. The Air is a magnetic medium. Weak magnetic bodies may be compared and measured in Sat. Sol. Sul. Iron through a considerable range. I do not think Plücker's observation will apply, for it has not been confirmed in liquids, as Sul. Iron, as far as I know. I mean his observation of difference of law of decrease and objection to my media.

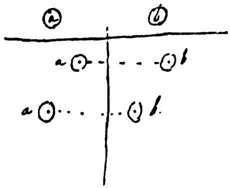
11002. *The Vessels containing the gases or other bodies.* Remember that the vessel acts powerfully when near, and complicates the result much. Where [Were] it not for the diamagnetic vessel, the Oxygen of 1 *a.* or $\frac{1}{2}$*a.*, or even of $\frac{1}{3}$*a.*, would have gone up into the Magnetic axis, or at least have hung against the iron core (10970, 1, 2, 3); and it is only when they are in the position they would assume if the gases within them and the medium surrounding them were the same, that they would compensate each other and not affect the result. If however the vessels be by the torsion restored to this equidistant state, then the torsion force indicated measures the relations of the two bodies alone and free from vessel error. So that the principle of measuring the forces is good here.

11003. If the vessels be inevitably different from each other, their diamagnetic force could be ascertained before or after the real experiment and then the correction made. It may be needful to have vessels with the expression of their diamagnetic force on them.

11004. Difference in the weight of the vessels or the weight carried by the torsion thread will make no difference in the measured result given by two bodies.
11005. The masses of bodies submitted at once to measurement of force ought to be of the same size and shape, and therefore the vessels ought to be of the same size and shape. Perhaps drawn thin copper tube would make good vessels.
11006. The vessels might be neutralized occasionally by the use of a medium of the same power, and that easily when they are made of magnetic glass, or brass, and that might be occasionally useful. They might perhaps be compensated by the association on the same stage with them of a feebly magnetic adjustible body, but I do not see that such means are required.
11007. The result of measurement, as I at present contemplate it, is a double differential result; for there are two differential results between the two bodies and the medium which they displace, and then there is the differential result of these two results.
11008. But if air be taken as one of the bodies and air be constant, then the differential result on one side is done away and that on the other side is between the body tried and air, i.e. either of the Air considered as medium or as in the second vessel, which comes to the same thing. Then Air would be the standard, and in that case bodies could be tried one by one against it.
11009. Or if the second vessel contain a vacuum, considered as Zero, then bodies can be tried in succession against it.
11010. The second vessel might be dispensed with altogether, and bodies tried single; and then of course there is only one differential result: that between the body tried and the medium in which it moves. But that does not appear to be nearly so convenient as where the Vacuum or standard medium is on the other side of the axis.
11011. Suppose two bodies in the balance—adjusted before hand equidistant from the axis, and the torsion index at Zero. Then the Magnetic force on and the bodies deflected—then torsion force on until the equidistant position is restored. The force registered will be the difference between the two diamagnetic forces of the bodies, provided the bodies are not allowed to open out but retain their respective positions; then if the vessels be

compensated and the one body be a vacuum, the force gives the power of the other body as a diamagnetic. But if the first body be not a vacuum but another gas, the force of torsion gives the difference of the two, both being diamagnetic.

11012. Suppose then that the force of torsion were added on until the position of the two bodies were reversed, i.e. until *b* should be as near as *a* and *a* as far off as *b* was; will not this second portion of torsion force be to the first portion as the diamagnetic force of *a* is to the diamagnetic force of *b*? For as *a* in the first position and *b* in the second position are subject to the same intensity of force emanating from the axial line, and also *b* in the first position and *a* in the second position are also equal in that respect, the power which can bring the system into the position of equi distance would seem to represent the powers of *a* and *b*. Now those powers are given by the two torsions. Consider this further.



11013. Should the index of torsion be at zero when the bodies are equi distant before the Magnetism is on, or afterwds. when the bodies are in their position of stable equilibrium? Having two forces, we start from a position of stable equilibrium and not, as in Coulomb's original expts., from a place determined by one force. Expt. will shew that the equidistant Zero is probably the true one?

11014. It appears as if the instrument involved no source of error and that the variations of power–distance–form, etc. all tended to give great facility of introducing more sensitiveness or more permanency or greater strength, etc. etc. without introducing any source of error.

11015. The vessels will require confinement so as to be prevented from opening out or altering their relative distance from each other, and there should be a provision for changing them from side to side, *though that would* NOT *compensate any error of position.*

11016. There should be a scale by which their true equidistance in relation to the magnetic axis should be carefully determined. To determine this position after the force is on is more important than before. *Before*, an error only involves a few degrees of torsion. *Afterwards*, it would involve a much larger amount of degrees.

11017. Shall be able to give a true account of the flame phenomena. It is probable that both with a flame and a glowing taper, far more diamagnetic effect is produced by converting the magnetic oxygen into diamagnetic C.A. etc. than by the heat; but I must make out what the heat does, for the platinum coil experiments shew that it does a good deal. Indeed, that is what I count on in explaining Atmospheric Magnetism.

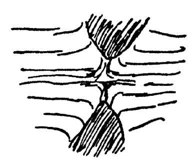
11018. Size of Grove's plates used with the Magnet. The full sized platina is $2\frac{1}{4}$ inches $\times$ 4 inches immersed in the N. Acid. The Zincs are $2\frac{1}{4}$ inches by $8\frac{1}{2}$ inches immersed in the acid.

11019. I must find if possible an experimental difference in the conduction of bodies more or less diamagnetic, including oxygen perhaps as a magnetic conductor; and ought to be able to do that, perhaps by some form of the torsion balance apparatus, occupying the field alternately by better and worse conductors. Perhaps a weaker magnet would be useful, and a permanent or constant magnet.

11020. I want a good and sure diamagnetic gas, one that by rarefaction tends less powerfully to *go out*, as Olefiant gas seemed to do (10978). That there are bodies diamagnetic, i.e. going out in a vacuum, is manifest, or else the tube of the Oxy. V. would have gone quite out and the *Oxy.* 1 *a.* or any other oxygen would have gone in.

11021. The little direct effect as in attempts at compression and expansion (10714, 30, etc.) and the great differential effect shews that, *in air* and the atmosphere, much may be produced by the latter action when little by the former. Probably important bearing in Atmospheric magnetism.

11022. Consider the two streams of Magnetic lines of force from the different poles. Look at them in two opposed similar poles. Is there any thing particular there which could be made manifest by a Magnecrystallic body placed in the center and in different parts—or would two at once act on each other.



31 JULY 1850.

11023. Have prepared some more glass vessels with gases like those before described (10968, 9), but the vessels are smaller and smaller, having taken the largest before, and are also very different

in weight, and the glass of the vessels goes for much in the comparison of gases differing only a little from each other, and the quantity of gas is also of *much importance*, so that comparisons with these vessels are very imperfect. The following is the continued list.

22. Mur. Acid Gas.	28. Coal Gas.
23. Ammonia.	29. Hydriodic Acid Gas.
24. Sulphurous Acid.	30. Chlorine.
25. Sulphuretted Hy.	31. Ether vapour.
26. Nitrous Acid.	32. Bromine vapour.
27. Cyanogen.	33. Sulphuret Carbon vapour.

All at 1 atmosphere.

11024. I lengthened the cross bar of the torsion lever so as to make the distance between 2 suspended tubes 2 inches or even more, and then hung up *Oxy.* 1 *a.* and *Oxy. V.*, 1 and 4 (10969), using the Magnet with 20 pr. of Grove's; but I did not find the effect better than when they were nearer, or quite so good. In fact, a certain extent of journey does not now take the gases and tubes through so great a change of force and hence diminished sensibility. Returned the tubes to the former distance of about 1 inch and then was good as before.

11025. Hung up the Various new tubes and others on one side of the core, *Nitrogen* 1 *a.* being constant on the other side–it was taken as representing *Zero.*

11026. *Nit.* 1 *a.* and *Oleft.* 1 *a.* (10969), 5 and 16. Very like each other. Oleft. smallest bulb.

11027. *Nit.* and *Cyanogen*, 5 and 27 (11021[1]). I think the Cyanogen *in* most, but there was not much difference in distance. Cyanogen the smallest bulb.

11028. *Nit.* and *Amm.*, 5 and 23 (11021). I think Amm. is nearest but only little visible difference–is the smallest bulb. Want the measuring instrument much (10991).

11029. *Nit.* and *Chlorine*, 5 and 30 (11021). Chlorine in probably. It looks as if it were magnetic and so had relation to oxygen–but then bulb is smaller than Nitrogen bulb.

11030. *Nit.* and *Bromine vapour*, 5 and 32 (11021). Bromine in more than the Nitrogen, but tube is smaller.

[1] Ref. in pars. 11027–11038 should be to par. 11023.

11031. *Nitrogen* and *Sulphurous acid*, 5 and 24 (11021). Apparently Equal.

11032. *Nit.* and *Mur. acid gas*, 5 and 22 (11021). Very like.

11033. *Nit.* and *Hydriodic acid*, 5 and 29. Very like.

11034. *Nit.* and *Sul. Hydrogen*, 5 and 25. Very like.

11035. *Nit.* and *Coal gas*, 5 and 28. Very like.

11036. *Nit.* and *Nitrous acid*, 5 and 26. Nitrous acid well in. Magnetic.

11037. *Nit.* and *Sulphuret Carbon*, 5 and 33 (11021). Sulrt. Carbon perhaps a little in, but the bulb was hardly half the size of the Nitrogen bulb.

11038. *Nit.* and *Ether vapour*, 5 and 31 (11021). Much alike—but the ether bulb both heavy and small, so glass much influence and ether vapour little.

11039. *Olefiant gas* 1*a.* and *V.*, 16 and 17 (10969). Hung up close to the Magnetic axis. They opened out and fell in, very like each other, as if there was not much difference—but then the glass is very influential in these places of strong action and short range of great variation in the magnetic force and may easily hide the gas effect, which probably is not great.

11040. *Oxygen.* Actual force of inset. Hung up *Oxy.* 1*a.* and *Oxy. V.*, 1 and 4 (10969) about 1 inch apart. The *oxygen* 1*a.* went in as before (10970). Then took a slender glass thread about 8 inches long, fixed at one end to a stand, and pushed the *Oxy.* 1*a.* back to its equidistant position by the other end; the glass thread or spring was deflected about 1 inch to do this. The glass thread was then fixed horizontal after 90° of revolution, so as to bring the flexure just obtained and that now to be produced by weights into the same plane as respected the glass, and then weight applied at the place which before had pressed against the oxygen thread. One tenth of a grain was not sufficient to cause a deflection of one inch like that produced by the inset of the *Oxy.* 1*a.* So this $\frac{1}{10}$ of grain is a rough mechanical measure of the relative Magnetic forces of *Oxy.* 1*a.* and *Oxy. V.*, and as the capacity of the glass bulbs was about 0·34 c.i., the absolute weight of that volume would be 0·117 gr. Hence the Magnetic force is not very far of[f] from its gravity under these conditions (11058).

11041. A rough estimate of the force is at once presented to the

eye by the Vibrations; and if two like tubes of like gas were hung up and the effect of the glass known, this would give an indication. But the resistance of the air is great.

11042. Two equal glass bulbs were put up, both having air in so as to give the effect of the glass only; they stood equidistant—but were very diamagnetic—opening out and also causing quick vibrations.

11043. *Phosphorous* as a *diamagnetic*. Filled a tube like that of *N.* 1*a.*, 5, with Phosphorous and then put it on the balance against the Nitrogen. Now as there is the glass tube to both, so the comparison is directly between Phosphorous and Nitrogen. The Phosphorous went out powerfully as a true and strong diamagnetic body.

11044. *Flint Glass*. Formed a piece of solid flint glass into the form and size of the *Nitrogen tube* 1*a.*, 5, and tried it against the Nitrogen. It went out powerfully as the Phosphorous, shewing the condition of it as a diamagnetic body. The instrument will measure all these effects well.

11045. In the Instrument. May perhaps be good to raise and lower the Magnet, instead of the torsion balance. Also one could easily *reverse the* MAGNET (rather than the balance and its charge), if there should be any object in doing so.

11046. In *Atmospheric Magnetism*, we must consider the atmosphere as a conductor into space, so that its power and influence is not merely confined to its own bulk and place, but influences and is influenced by the medium or space beyond it.

11047. Does the magnetic condition of Space indicate any relation to the supposed ether, or to any other condition of space equivalent to it?

11048. What a strange Magnetic system our Planet presents. First the Earth itself as a magnet—and that not unchangeable, for it is probable that the secular variations may be due to some alteration of its internal or inherent force. Then Space as a great and good conductor of the power, and probably permeated by the lines of force to a great distance from the planet. If a globe have currents of Electricity running round it, it can affect a very sensitive needle at distances equal to 3 or 4 times its diameter. So the Magnetism of the Earth through space may extend to three or

four times its diameter, or 30,000 or 40,000 miles or more. Then comes between this floating atmosphere of Ox. and Nitrogen, rising not 50 miles high with sensible power; yet within that distance by its oxygen having great power, and forming by changes of temperature an ever varying magnetic medium between the Earth and the space above it. It seems impossible that this can be so and not produce numerous magnetic Variations but sudden and annual.

11049. Assume that oxygen has only half the magnetic force of iron, weight for weight (11040); then the oxygen in a vertical column of the atmosphere of 1 square inch base= 3·33 lbs., and half of this or 1·66 lbs. of iron would represent the Mag. force of the Oxygen in that small column. This would be equal to a column of iron 1 inch square in the base and 6·32 inches high. A very great expression of force. But oxygen is not nearly so strong as this (11066).

11050. Is there any possibility of large masses of hot and cold air being *moved* by terrestrial magnetism in consequence of a change in their magneto conducting power? I think there is.

11051. *Atmospheric magnetism.* Have to consider disturbances in *intensity* and disturbances in *direction.*

11052. According to the observations made on terrestrial magnetism, the change in direction is very small. What is the amount in degrees of the daily variation and the Annual variation? Also the variations in intensity are very small indeed. What is the amount of variation compared to the whole power exerted?

11053. If change of temperature quickly affects oxygen, then two bulbs, of oxygen and air, ought to move and indicate changes of day and night temperature, and summer and winter, using an artificial magnet and rapidly decreasing field of Magnetic force.

11054. The Aurora borealis may now become connected with magnetic disturbances and storms in a very distinct manner; and if the variations of the atmosphere cause both, it will also tie both together by a common hub.

11055. The place of the Zones of Aurora Borealis and Australis may now be recognized as dependant probably on the temperature of the air in those regions in relation to the temperature nearer to the equator.

11056. As we rise in the atmosphere, the oxygen becomes rarer and for that reason less Magnetic (10972), but it becomes colder and for that reason retains its magnetism more. Here therefore come in other causes for Variations of Atmospheric magnetism. The locale of the chief disturbances is probably confined to a very limited stratum of air, perhaps not more than 3 or 4 miles high, i.e. the locale of the causes of the disturbance. The disturbances themselves may extend to a far greater height (11048).

11057. I must refer to Sabine for the lines of force—their direction—their intensity—and their different subdivisions—Height in the atmosphere and diminution if known—Daily variation—Annual variation—Variation at St. Helens[1] or on the sides of the equator and others if there be such.

3 AUG. 1850.

11058. *Volume* and *weight* of *Oxygen* 1 *a.* (10969). The bulb displaced 86 grains of distilled water, which is equal to 0·34 of a cubic inch—it includes of course the bulk of the glass, which is very small, but if considered all as oxygen then it equals 0·117 of a grain (11040).

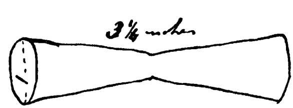
11059. Two small iron balls, about $\frac{1}{12}$ of inch in diameter, were put into a field of equal force $\frac{1}{8}$ wide and about $2\frac{1}{2} \times 2$ inches in extent, to ascertain how much they would repel each other. This repulsion, which of course was in a direction transverse to the lines of force, was very considerable, so that it required a considerable push with a card to bring them together; and when they were let go they flew asunder not a mere trifling distance but from half an inch to more, and if they had been quite round, I have no doubt would have gone an inch or more asunder (10884, 93, 4).

11060. I must hang a bismuth sphere in a field of *equal force* in a sol. of Sul. Iron by a delicate and shaded torsion balance with a natural magnet, and then bring another equal bismuth sphere first on one side and then on the other. If it moves, it will not only shew the action correspondant to iron but also the effect of the conduction of the bismuth (11104).

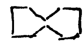
11061. Have had a soft iron core made of this form*, and now put it in place in the electro magnet (10925) in place of the former, and hung up the Ox. and Nitrogen V and VII (10905) to see

[1] Query St. Helena.

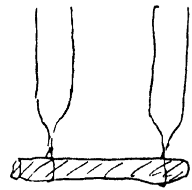
* [11061]



how the core acted, using the Magnet urged by 20 pr. Grove's plates. The action was right, but feeble as compared with the former core, so replaced this core and then the gas bulb told as before, strongly. Perhaps this core might be improved by turning away a little of the middle part, i.e. making the neck narrower.



11062*. *Used water as the Medium.* Had a glass cell $\frac{3}{4}$ of an inch wide, 3·2 inches long and 4 inches deep inside—placed this in the magnetic field and then brought my pointed poles up to it, so near that they were $1\frac{1}{4}$ inches asunder from each other. The Oxygen V and Nitrogen VII ( ) had little copper rings attached to the bottom and were ballasted by a thick piece of copper wire to sink them in the fluid, and were $1\frac{1}{2}$ inches apart counting from the central line of each. When the magnetic force was on, the tubes obeyed but very slowly—for the poles are far apart—the lines of force not strong as before—the decrease in their power not so rapid as before and the resistance of the medium great. The effect was poor and offers no advantage in the construction of an apparatus.



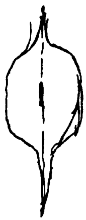
11063. Then replaced the water by a saturated solution of *proto sulphate of Iron*, and now the tubes obeyed very well, opening out and collapsing as the Magnetism was on and off. But because of the nature of the medium, they took their places so nearly equidistant as to give but little indication of the difference of oxygen and nitrogen, the iron solution and the glass of the tubes overpowering the gas effect. When the *nitrogen* was pressed inwards and left, it went out immediately, and when the oxygen was pressed in and left, it also went out at once; i.e. the tubes took up their position of stable equilibrium easily.

11064. Of course the most delicate arrangement is when the objects are in nature near to the medium; for then, of the three differences between the medium and the two objects, those between it and them is less and that between them greater in proportion to each other.

11065. I put the tubes V and VI, i.e. Oxygen 1*a.* and Oxy. *V.* into the sol. sulphate of iron with the same result as the *Oxygen* and *Nitrogen.*

11066. *Oxygen compared with iron* (11049). The oxygen in tube 1 (10969) weighs 0·117 of a grain (11058). I took a fine iron wire

* [11062]



of which 0·448 of an inch equalled 0·117 of a grain, and twisting a fine platina wire around it, I fixed it in a Glass bulb like the oxygen bulb, so that the iron (which being doubled was 0·25 or thereabouts in length) should be in the middle—the bulb containing a nitrogen Vacuum. Then hung it up against the corresponding Oxygen 1 *a.*, No. 1. Of course the iron wire was transverse to the magnetic radius and in the equatorial plane, and therefore very little extension given to it in the direction of the lines of force. This was arranged to diminish as much as possible the effect of polarity. The iron was *very far* stronger than the oxygen and caused its bulb to run up and rattle against the side (11103).

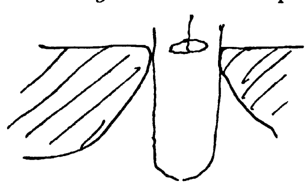
11067. *Oxygen and sol. Sul. Iron compared.* Choosing a bulb nearly the size of the oxygen bulb No. 1, I filled it with solution of proto sulphate of iron, more or less diluted until its power was nearly like that of oxygen. When the solution contained 1 part of the saturated solution and 31 parts of water, the oxygen was most magnetic. When it contained 1 part of the solution and 23 parts of water, then I think the solution was a little the strongest—perhaps $\frac{1}{26}$ part saturated solution might be about right. When tested by Amm. or Per ferro pruss. pot., the quantity of iron in the solution appeared to be very great (11103).

11068. Took the two bulbs *Ox.* 1 *a.* and *N.* 1 *a.*, 1 and 5 (10969) and observed their action when hung up *alone*, that I might get an idea of the power of the glass. The *Nitrogen bulb* went every where. The *oxygen alone* also driven out every where, the diamagnetic power of the glass being so much more than of the oxygen within. So the manner in which the two bulbs neutralize each other as to the glass very striking, since being so strong, still the difference of the Oxy. and the N. when both are up appears so well.

11069. Still, the bulk of glass which can overpower the oxygen when that bulb alone is up is very small compared to that of the gas, and its power therefore very great. It would be a great point to have it nearer to the gases or rather perhaps to the medium, so as to get rid of its effects more or less. Perhaps copper would do, or brass. Vessels at Zero of quality would do.

11070*. *Bismuth* in *Nitrogen Vacuum.* I suspended a little piece

* [11070]



of bismuth in a Nitrogen Vacuum in a tube about 0·75 of an inch in diameter and then brought it between the poles of my terminals. It obeyed the magnetic force very well.

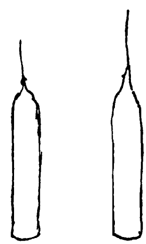
11071. *Iron.* I suspended a piece of Iron in the same manner in nitrogen vacuum; it also obeyed as well as in a plenum, and pulled the tube about in the hand with great power as it flew from one side to the other. It could not be kept in the middle of the tube except in lines of weak action, and then tended by the least derangement to fly to the side.

11072. The magnetic power of iron is wonderful and what may be considered its conducting power. How many lines of force must of a sudden be determine[d] on to it, or on to the space it occupies, by its presence. Yet all that almost taken away by mere heat.

11073. A bismuth cylinder $2\frac{1}{2}$ inches long and $\frac{5}{8}$ of an inch in diameter was prepared and also a glass tube just able to hold it, and the tube being filled with water, the cylinder was put in and also a guage (10715), so that the fluid should appear in the very narrowest part. This was then placed in the Magnetic field (10857, 8, 9), and the power put on and off; but not the slightest trace of change of volume appeared.

11074. An iron cylinder of the same size was fitted up in a similar way, but the pressure of the portion within first deformed and then broke the glass tube, so that no result could be obtained.

11075. Two equal cylinders of the same glass were prepared and platina wires attached to them by fusion. Being hung up on opposite sides of the core and subject to the Magnet, they appeared to be quite alike in force. Then one was made very hot and both were again hung up. Perhaps the hot one went out a little, but I am doubtful. The hot air about it ought to cause it to go out, but then the mass of glass is great and that may have overruled the effect. Still, that would shew that hot glass is very little different in power from cold glass, since the whole effect was so small as to be imperceptible.



11076. *Atmospheric magnetism.* Probable variations of force on the earth's surface and their causes.

11077. As the lines of force are horizontal at the equator, and the temperature higher there than at the Poles, so the intensity of the force ought *on that account* to be less there than at the poles.

11078. As difference of temperature below and above is greater there than at colder parts of the earth, so the intensity below and above ought to differ more than in colder places.

11079. Probably as change in temperature at a given spot is less there than at the tropics, or north or south of tropics, so the annual variation ought to be less.

11080. The comparitive constancy of low temperatures at the poles. What will that do?

11081. The Annual Variation and the daily variation, will they not be more regular and constant over the Sea than over the land?

11082. The atmosphere *by its oxygen* is a far better Magnetic conductor than the space above it. So this ought to have its effect in conducting the lines of force through the places where it exists, and probably also has a different kind or amount of effect according as the lines of force tend to be parallel to it, as towds. the Equator, or tend to be transverse to it, as in these latitudes and towards the poles.

11083. Even *pressure of the atmosphere* and its Variation may be an element of magnetic variation, because of the difference in the amount of air and therefore of Oxygen over a given place or space.

11084. Winds and atmospheric currents may be an element of Magnetic variation, by tending to mix up or change the places of Masses previously of different temperatures and therefore of different conducting powers.

11085. The Moon is only   miles from the Earth—and is possibly within the reach of its lines of force. Are they of such force there, and it of such a nature as to be likely to cause any variation by its change of place? It is Volcanic and all Vo[l]canic

rocks are Magnetic. What would it do if it were a mass of iron? Any thing sensible to us?

11086. Instrument (measuring). The *torsion wire*: the silk has too little torsion for the torsion measure—Glass I suspect would have too much unless long and slender, and then inclines to break—Platina wire hard drawn would probably be a good substance.

11087. Instruments to shew varying position with temperature or otherwise should have suspension with *No torsion*; hence value here of cocoon silk. Instruments for measuring the deflecting force by the torsion should have *torsion filament*, with strength enough and torsion force; and if too stiff, increasing the leverage of the carrying arm will give more power on the torsion index.

11088. As to horizontal bar and its length, the increase of suspending arm give sensibility by increasing the force on the torsion axis. The other end might be made a long index by a piece of hay or other light thing and perhaps do well instead of a mirror, i.e. for the measuring instrument.

11089. There should be a moveable stage under the middle of the torsion lever, both to carry it and also to steady, adjust and deliver it free from motion.

11090. The torsion thread, lever, etc. should all have four or five inches journey up and down in a vertical direction to relieve the vessels when needed from the magnetic axis. As this motion should be truly vertical, so there should be a Plumb line or a level or sufficient means to set the instrument accurate.

11091. The *reflector*, if used, may be a piece of flint glass silvered by the new process—or a piece of planished silver made slightly concave, as a cylinder. It may if plane be under the torsion thread. If concave, it would of course be useless if placed concentric to the axis of torsion, and become more and more sensible as it was brought nearer to the eye.

11092. The medium is evidently important. If very different from the objects to be compared, it diminishes their sensibility (11063).

11093. The air is probably a good medium for Gases, being more magnetic than Zero by the fifth of Oxygen which it contains, i.e. a good medium when Nitrogen or a Vacuum is the standard object with which comparisons are to be made in it.

11094. The Vessels to hold gases or other bodies, and the stage carrying them, should be as near Zero or air as may be.

11095. Would be well to compare metals and substances to find the best.

11096. An instrument might be made with the Bar of substance setting equatorially, and if Reisch's iron were below it would probably set pretty well, and the force which would deflect it to 45° or any other number might be used as the measuring force. But I do not think it would equal the former instrument with the bodies on opposite sides of the magnetic axis.

11097. May apply the instrument to determine whether Plücker's difference in the rate of increase or decrease of Magnetic and diamagnetic force is so or not—and if so, whether it is confined to polar Terro magnetic bodies or extended to others, i.e. whether a distinction may be drawn between Terro magnetic polar and non-polar bodies.

11098. Also to ascertain whether diamagnetics can become Terromagnetics by increase of force, as some say. If such an effect occurs, must ascertain whether the oxygen of the air has any thing to do with it.

11099. Also whether the oxygen of the air has any thing to do with Plücker's difference of decrease and increase of force in Terro magnetic and diamagnetic bodies.

11100. Also determine directly the law of the increase or decrease of force with a given shaped magnetic core at different distances from it, by using two equal bodies and carrying them by torsion to unequal distances.

11101. I ought to be able to adjust the place of the torsion film so as to know when it is in the normal position, i.e. when the bodies supported by it are equidistant from the magnetic axis, no magnetic power being on.

11102. Can I not obtain this adjustment and the position of the Zero point by using *like objects*, and adjusting the suspension and the torsion to them when in place—using the prolonged index or the reflector to indicate that is at place of Zero?

11103. Continued the experiment of comparing iron directly with oxygen (11066) and successively reduced the little piece of iron to one fifth and one tenth of its former quantity. But it always powerfully overcame the oxygen and the glass vessel, and actually bent the platina wire which sustained it in the center of the bulb. So that, as might be expected, there is no numerical comparison here between platina and iron: but the comparison is as yet only between 10 of oxygen and 1 of iron by weight.

11103 *b*[1]. *Oxygen and Sulphate of Iron* compared. 100 grains of clean good crystals of Proto sulphate of iron were dissolved in distilled water with a drop of Sulphuric acid, and diluted until, by comparison as before (11067), it had power apparently equal to oxygen gas. The bulk of the solution was then 17½ cubic inches—so that is the character of the equivalent solution. The two vessels or bulbs were not alike in shape, but assuming that they had equal capacity, then as the oxygen bulb was 0·34 of a cubic inch, so its volume or 0·34 of a cubic inch of the solution would contain very nearly two grains of crystallized sulphate of iron = 0·4 of a grain of metallic iron. So here the oxygen is equal in force to a solution containing 17 times its weight of crystd. proto sulphate of iron, or 3·4 times its weight of metallic iron in that state of proto sulphate.

11104. Arranged the Glass cell and solution of Proto sulphate of Iron, saturated, as before (11062, 3). Suspended a cylinder of pure phosphorous, 1⅛ inches long and ½ inch in diameter, vertically in it, in a field of equal force (10917) at the Magnet (20 pr. Grove's plates) by the torsion balance. The phosphorous remained perfectly still in the middle of the solution and magnetic field. Then placed a corresponding cylinder of phosphorous, supported by copper wire, in the solution by the side of the first, and when all was at rest put on the magnetic force. There was action between the two diamagnetic bodies at once, for that on the torsion balance slowly receeded from the fixed one. This took place again and again, with every precaution. The action is very feeble but still distinct, and consists in a separation of the two bodies. It is therefore *like* that of iron (11060, 11184).

[1] There is no 11103*a* in the MS.

11105. Does not this exhibit a *case of conduction* in phosphorous as a diamagnetic (11060)?

11106. *Instrument.* Torsion suspensions. I have made a bundle of 180 films of cocoon threads—it is $2\frac{1}{2}$ inches long. When hung up and a card lever attached to the lower end, the arms of which are 2 inches long, the lever is turned through and held at a torsion of 360° with very little flexure of the glass spring (11040)—not equal to $\frac{1}{50}$ of a grain. This would not be enough for my purpose, and also as the bundle is twisted the threads change their place and the whole bundle its diameter, so that the forces are not equal or regular. Silk is a beautiful substance for suspension, free from resistance of torsion—it is almost like suspension by floating.

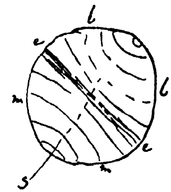
11107. A glass thread was drawn 18 inches long and of a reasonable thickness for strength. Being put up, it was tried by the 2 inch leverage and glass filament. But now a deflection force equal to one inch or the 0·1 of a grain (11040) could scarcely deflect the torsion lever 10°. So far too little liberty here. This filament held 4800 grains and then broke. No doubt Green glass would be stronger and might be drawn finer and longer to give a greater range of measurement; and so glass may do for certain substances which are light or for certain cases. Its elasticity is perfect and there is no fear of set. But it is very liable to accident.

11108. *Platina wire from Newman.* A length of 8 inches put up with the card lever. It could be sent round or held at torsion of about 270° by the inch deflection of the glass filament, or force of 0·1 of a grain (11040). This is a good range. But the metal is apt to set, for when held round at 360° for 15 minutes and then relieved, it had lost about 25°. However, I had made it red hot to straighten it; if pulled it would be harder, or if taken as it comes from the draw plate. It could support 3200 grains well without breaking. 9 feet 6 inches weighed 4 grains.

10 AUG. 1850.

11109. *Atmospheric Mag.* As to the annual variation, considering it as depending more or less upon the oxygen of the atmosphere and its variations in temperature. The effect of the revolution of the Earth in its orbit is to effect a considerable

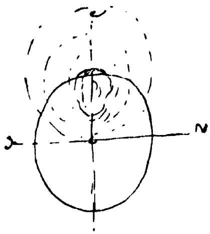
change in the temperature of the earth's surface in the Northern and Southern parts, and but little at the Equatorial parts. Because of the daily rotation of the earth this change would come on generally, i.e. over the whole of each hemisphere at once and not as a flux from East to West; it has nothing to do with the daily variation of aspect to the Sun, except as that produces this general effect once each hemisphere. So in Summer, the northern parts of the earth and therefore of the atmosphere above is warmed, and in Winter they are cooled; and a given plane of temperature, as that of perpetual frost, is rising in the northern parts and sinking in the Southern parts during our Summer, and on the contrary is sinking in the northern parts and rising in the Southern parts during our winter. Now what ought this to do?



11110. In summer, the atmosphere about the equator *e* ought to conduct Mag. as in winter. But in summer, the parts about *l* on each side ought to conduct worse than the corresponding parts about *m*, the Southern latitudes; but how far this effect will extend towds. the poles will depend on the effect of cold as we shall find it to be. Must try that by experiment. But generally, the north part of the Northern hemisphere will lose in conducting power, whilst the South parts of the Southern hemisphere are gaining during our Summer–and in the winter our North parts will gain power and the southern parts lose.

11111. Therefore there will be less conduction of Magnetism from and through the Northern hemisphere in the summer than in the winter, and more in the Southern hemisphere; and this state of things will be reversed in the winter. This ought to cause a variation even at the Equator. For as the surface of north discharge becomes less effective in intensity, it must become more extended to compensate for that deficiency, and as in winter it becomes more effective from increased capacity of conduction, it will become smaller, i.e. more concentrated. A wave of Magnetic force or condition must pass from one hemisphere to the other across the equator in summer and winter.

11112. But as the equatorial parts will remain nearly the same in condition, this wave will perhaps not appear there but at the edges. Thus as the north parts increase in conducting power in



winter, so the middle line *c e* of the curves at the equator will lean over towds. N and consequently the dip where it abuts ought to change a little—and the contrary in the opposite season. There ought perhaps to be a small diminution in the dip in the northern hemisphere, increasing until the effect is a maximum and then decreasing until it is a minimum 6 months afterwds.; and at the same time a contrary effect ought to occur at the other hemisphere.

11113. Also as the dip increases, the intensity of the magnetic force ought to increase; and as it diminishes, the intensity ought to diminish.

11114. Perhaps no other annual change than these two ought to occur.

11115. As the one hemisphere becomes colder and the other warmer, the former ought to have a greater weight of atmosphere over it, the upper current on to it being more abundant than on the warmer side. Is the difference in quantity at any time such as to make a sensible annual Magnetic effect?

11116. There is less air over the Equator than over the Poles, as the barometer and other considerations will shew. What influence will this have in the general distribution of the lines of magnetic force over the earth?

12 AUG. 1850.

11117. The oxygen in the air and therefore the air (if a mass be heated or cooled) become polarized as conductors either Terro magnetically or diamagnetically (11130). If the air be cooled, it is polarized as soft iron would be polarized in the same position.

11118. So may use of [? a] bar of soft iron with a pocket compass below it to indicate the probable effect in different positions of a mass of cooled air above. Perhaps a large cylinder filled with solution of Sulphate of iron might produce the effect—or even a cylinder of oxygen heated and cooled. Try it.

11119. The *Annual Variation* may be affected by the difference of land and Sea, *more* change taking place over the former than the latter, so that it may differ in places where on other accounts it might be expected to be alike.

11120. The same causes (difference of water and earth) may also produce, at the edges of the portions concerned, variations in the horizontal direction as well as in the dip and intensity.

11121. The daily variation by itself should be one effect from Maximum to Minimum and back again to Maximum in 24 hours.

11122. *Diurnal variation.* In England, the North end of the needle moves slowly eastward during the forenoon—returns to mean position about 10 o'clk. P.M.—goes west—and returns to mean position about 10 A.M.

11123. Kupffer says there is a small nightly variation.

11124. Causes of the irregular variations. Winds very influential. Hot and cold masses of air—well known. Precipitations of rain or snow, evolving heat above.

11125. First one has to consider what are the fixed lines of the Earth's magnetism (without any of the superinduced variations) for a certain moment of time. These are very irregular and are approximately given by the average lines on magnetic charts.

11126. What are the causes of these fixed irregularities? Very probably difference in conducting power in different parts of the Earth's mass is one great one. Perhaps that view of the Polarity of the Earth's particles which Whewell attributed I think to Hansteen may include the effect of these causes in what he considers the common cause of the whole magnetism.

11127. Then come the slow variations which have no reference to astronomical periods, as the motion of the line of no variation and of course of the other lines. These seem to be due to something internal as respects the earth, and may well be so. The motion of the line of no variation is, I understand, irregular—but that may depend, not upon irregularity of the moving cause, but upon irregular fixed conducting parts of the earth which are between what we may consider the real cause, or seat of the cause, and the surface. The varieties of Geological formation must do something of this kind.

11129[1]. However, I purpose dealing only with *Atmospheric variations.*

Then come on the *Annual variations.*

Then come on the *Daily variations.*

And then come on the *irregular variations.*

11130. For the purpose of having a type of the probable changes in direction of the needle from the causes I assume: conceive a

[1] There is no 11128 in the MS.

globe of air in the atmosphere above London, lat. , and the condition it would come into and the effect it would produce on a needle beneath it; and for convenience of reference and also of trial (11154), let us conceive it to be made colder than the surrounding space. It will then be a better conductor and will be polarized as a Terro magnetic body (11117) like T*[1]. Now what effect will this Globe of air have upon the *intensity*, the *inclination* and the *declination*, as shewn by a magnetic needle placed in different positions in relation to it?

11131. If a magnetic needle were taken from *a* outside, on a level with the middle of the globe, along a horizontal line, *a* being supposed to be a part of the general space where the lines of force have a certain dip and intensity, then *a* would give the normal condition of intensity; at *b* it would be less, and also at *c*, because of the condensation of force at T; at *d* it might be normal again and at T more intense. As it went onwards, corresponding variations would occur on the opposite side of T.

11132. On taking the magnet upwards from *e* by *f*, etc. in the line of the dip, the intensity would gradually increase up to T and then gradually diminish. If taken up perpendicularly in these latitudes, it would be nearly the same. But it is clear that as we depart from the dip and approach a line perpendicular to it, we shall gradually have the variations before described. Hence at the Equator, a magnet taken perpendicularly upwards would shew the latter more complicated variations.

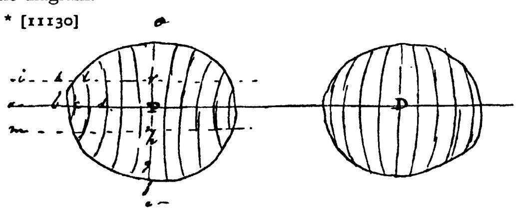
11133. As regards the *intensity*, Hansteen says that the intensity has been decreasing at Christiana, London and Paris at the rate of $\frac{1}{235}$, $\frac{1}{725}$ and $\frac{1}{1020}$ part of the whole force. Also

11134. Humboldt and Bessel say the *intensity* of the Magnetic force increases from the Equator to the Poles—and is doubled from the Equator to the western limits of Baffin's Bay.

11135. Theoretical variations of the *inclination* or *dip* by such a globe of air as T (11130). The dip would not vary in a central line parallel to the dip. Along the line perpendicular to the dip, as the line *aT*, it also would not vary; but along any line lying in a plane perpendicular to the dip and above the center of the

[1] T in this and succeeding paragraphs refers to the symbol resembling P in the diagram.

* [11130]



sphere T, the needle would as it passed from *i* to *k* have its lower end carried inwards towds. the *central* line of dip T, that effect coming on at *k*, continuing at *l*, and when it had arrvd. at a maximum effect, diminishing again until the dip was of the normal amount. So if the needle be supposed to be carried round the central line *Te* of dip in a circle concentric with that line, therefore having its circumference passing through either *k* or *l*, the needle would describe part of a cone having its apex below.

11136. On the other hand, if the needle were in a plane below the center of the sphere, as *m*, *n*, etc., it would undergo corresponding changes in the reverse direction; its upper end would now point inwards or towards the central line of dip and the cone it would describe would have its apex upwards.

11137. So the dip would be altered or vary in every azimuth, and it would vary in opposite directions in the upper and lower parts of the Globe and the affected surrounding space.

11138. The above details relate to the globe of air itself, or at a place where the dip is vertical or nearly so. When the dip is nothing, i.e. the lines of magnetic force are horizontal, as at the equator, then the same changes occur relative to the globe of air but not relative to the earth. A card model in different positions on a globe readily shew[s] what these changes will be.

11139. In respect of the dip, Sabine says it has been decreasing (here?) for the last 50 years at the rate of 3′ annually.

11140. The *declination* variations of course depend upon the same changes of the directions of the lines of force as the *dip*. In a plane above the middle, as *i*, *k*, *l*, etc., there is a force tending to make the lower end of the needle go inwards (11135), acting as if a S pole or the south end of a magnet were below the plane in the line *ef*. In a plane below the middle, as *m*, *n*, the upper end of the needle tends to go inwds., being affected as if an N pole were over it in the line *To*.

11141. So in these latitudes the effect of the globe upon a declination needle placed beneath it might be represented experimentally by a N pole placed over it in that line which is considered as passing through the place of the center of the globe—the S pole of the Magnet used for the purpose being still further off in the prolongation of the same line.

11142. A similar arrangement will indicate the variations at the equator and in intermediate latitudes.

11143. So the intensity may vary without the dip, as along the line *Tge* (11130, 11132), or with the dip, as along the line *abcdT*. As respects the dip, positions could easily be traced out where the declination should change without the inclination, and the inclination without the declination; so that inclination, declination and intensity may all change alone or in any kind of combination with each other.

11144. In applying these principles to natural phenomena and searching for their correspondence, we have to consider that the atmosphere diminishes in oxygen matter upwards in consequence of its rarefaction, and that therefore in that respect it does not agree with a supposed globe of uniform constitution. Again, that the heated portion of the atmosphere in nature will never be spherical, but perhaps more like a dome, or very irregular in form. So that the needle will most probably have to be considered as in a plane towards the middle of the sphere; or if the earth be supposed to be affected only by the action of the warm or cold air altering the lines of force, and not directly by the sun, then the acting mass may probably be considered as a flattish portion and the needle at the bottom of it.

11145. It will also be important to refer what may be called the dominant line of the mass, or that representing the central line of force of the Globe (11130), correctly to the principal line or surface of section of the advancing or moving volume of warm and cold air.

11146. Also, in applying these principles, must remember the variations due to Sea and land (11119). I do not mean their permanent effects, but the effects of more and less cooling and warming as the sun comes over them annually or daily.

11147. In nature we have power to observe in one plane as regards the Earth, which being *very irregular* in form and position, is *very unchanging*.

11148. The quantity of power required to move the needle for declination is very small in these latitudes, and I think that given by the cause ought to be enough and will be enough. The quantity of power required for the same purpose at the equator is much

larger, for there the declination needle is parallel to and in the magnetic line of force, and has on it what in our latitude is divided into declination and inclination force. For this cause and also because the changes of temperature are less in extent at the equator than here, there ought to be less daily variation there than here. At least I think so, but how is it?

11149. Gay Lussac and Biot in their balloon ascent observed a perceptible decrease in the intensity of the magnetism above. This might or might not be a pure result of distance from the Earth. It may have been influenced by temperature. At what time of day was their observation made, and where would they be in regard to the air temperature consequent upon the Sun's presence over the earth?

11150. *Annual variation.* Should that include the effect of cooler and warmer seasons? Such an effect must come in somewhere and probably there.

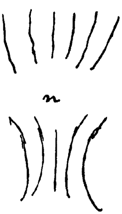
11151. What is the final purpose in nature of this magnetic condition of the Atmosphere and its liability to annual and daily variations? No doubt there is one, for nothing is superfluous there—there are no fag ends or surplussages of action there. The smallest provision is as essential as the greatest. None may be wanting.

11152. Wonderful to observe the physical constitution of oxygen in the atmosphere, besides what concerns its chemical action. This its magnetic condition. And then Nitrogen, so different in many things and in this—and also in its relation to Static Electricity or flashes of lightning. Oxygen does not seem to have this magnetic constitution in common with other bodies of the same chemical character, for chlorine, bromine, cyanogen, etc. appear very different to it.

11153. May make very good and useful Card models of the direction of the lines of force in a globe of air or in masses of other forms, and they serve important investigating or indicating means, in association with a terrestrial globe.

11154. Just for a rough beginning of comparison with nature: let us suppose that it is midday here, and that the Sun acts at once in heating the earth's surface and the air above. Then at that time there will be the lower diamagnetic pole (   ) over a

needle here. At Midnight, there will be a lower Terro magnetic pole passing over the needle here, and we may conceive that from 6 A.M. the diamagnetic pole is coming on from the East, then passing away Westward until 6 P.M., at which time the Terro-magnetic pole will come up until Midnight and pass westwd. until 6 A.M. Now the lower end of a soft bar of iron is such an N pole and is even produced in the same way, as I suppose. As it comes up from the East, it causes the N end of the needle to pass Westwd., until it is nearly over it, and then as it passes over, the Needle quickly returns to 0° and as quickly goes over it to the E., and then as the pole passes on the needle moves back from E. to the Zero point.



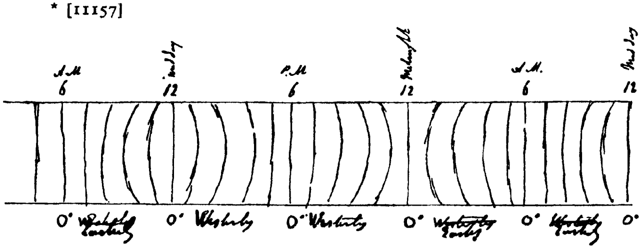
11155. To represent the passage of the hotter period or portion of air, I want a diamagnetic pole passing over the needle from E. to W. Now may not this be represented by a S. pole or the upper end of the poker passing from east to West under the compass? For a diamagnetic pole above has the lines of force converging downwards and a Terro magnetic pole below has the lines of force in the same direction. So I think there is no mistake here. I do not see how there can be, since I really have to do with Terro magnetic and diamagnetic conduction poles (11130).

11156. Now, the poker passed with its upper end from East to West below the compass makes the N. end pass E., then when poker is near, move with it and return to Zero; then move on with poker to West and, as the poker goes away, return to Zero.

11157. The directions of the lines of force which pass over the needle are as follows*. This would give four zero points and two sets of variations.

11157½. But considering the condition of the air on the face of the globe, and that a hemisphere is always exposed to the sun's surface, it seems more probable that the lines of inflexion belonging to the two opposite conditions of hot and cold are not separated by parts of the air uninfluenced by either, but that the curvatures of the Terro magnetic and diamagnetic states run into each other

* [11157]



thus*. Indeed it must be so, for the action of both hemispheres must be considered at once on the lines of force. Then there will be only two zero points, here set down for a rough approximation at Midday and Midnight, and the needle would point East of medium position from Midday to Midnight and West of it from Midnight to Midday. It is said to point East from 10 A.M. to 10 P.M. and West from 10 P.M. to 10 A.M. (11122). This is a very encouraging approximation for a first idea.

11158. There may be many causes which may make the Zero condition come on at 10 o'clk. rather than 12 o'clk., but must first know what are the hours and degrees of Daily variation over the earth's surface, for the problem to be solved is not confined to London.

11159. *Instrument*. The lever arms ought to shift each for itself and hold rather stifly. The Straws answer very well.

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11160. Instrument. A copper cross, dropping from under torsion line into vessel of clean water, to stop vibration and steady the instrument and save time.

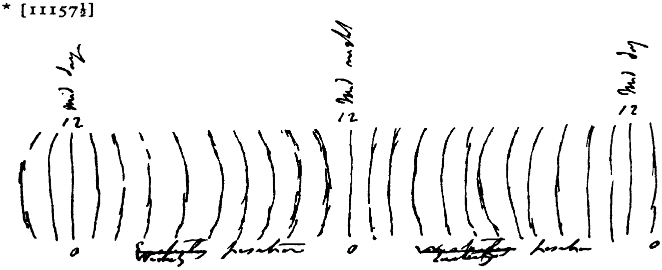
11161. Globes or portions of Air separate from the surrounding air in temperature, quality, etc. do occur, *Prout*. So may fairly reason on their occurrence and effect.

11162. My results give reasons for the occurrence of double variations—or of two Maxima and two Minima (11157, 57½) and these either large or smaller. According with Broun's observation on the Daily variation of intensity. Edin. Phil. Trans., XVI, Part II, page 99.

11163. Daily Variation—or irregular variations affected by the manner in which the Sun's rays are or are not interrupted by clouds. Its heat located in different places both in height and over the face of the Earth, according as these exist or are formed or pass away, etc., etc.; and hence great variation of portions of the air to small amounts.

11164. Yet every variation must have an effect, whether sensible

* [11157½]



or not. Perhaps it may be possible even to observe the effect of a single cloud, as it passes over the *line of dip* in a needle, when it is deep in vertical dimensions. Or perhaps observe the effect of the transparent warm portions spoken of before (11161).

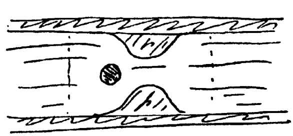
11165. Would it not be possible so to arrange a magnetic needle, by the counter action of magnets, as to make it more sensitive; i.e. so to adjust the curves of force passing through it as to make their variation greater with a given charge above than it is now.

11166. The great horseshoe Magnet with *field of equal force* occupied by solution of Proto sulphate of iron as described (11062, 11104). Then a ball of *bismuth*, 0·25 of an inch in diameter, suspended from a torsion balance so that it could only move axially in the Mag. field. It stood indifferently when the Magnetic power was off, but when it was active, then the ball went to the middle of the magnetic field, exactly the reverse of Iron (   ). It did so from either side of the field and very freely.

11167. Had a wider cell of Sul. Iron $1\frac{1}{4}$ inch across. Now also did the same thing, but not so strongly.

11168. Then placed a second sphere of bismuth as a fixture by the side of the former (11166) in the direction of the magnetic axis, to ascertain whether there was any mutual effect; but could not discover that there was. Placed a mass of phosphorous in the position of the second bismuth—still saw no attraction or repulsion in the line of the axis. I think it probable that if two pieces of phosphorous were placed as p, p*, they would weaken the lines of force in those parts, and that this might be shewn by a ball of bismuth or phosphorous tending to go up on either side to between them. If it did, it would shew badly conducting power of p, p (11181).

11169. Small Calc. spar cube with Optic axis perpendicular to two of the terminal planes. Suspended it by cocoon silk with the optic axis horizontal in a field of *equal force in Air*. Immediately that the Magnetic power was on, it set well with Optic axis equatorial, and on either side. The effect was as it ought to be and very good for a magnecrystallic effect. That the little copper band which surrounded it might have its effect eliminated, I turned its position 90° on the cube, but the cube set as before, the copper band being at one time axial and at the other equatorial.

* [11168] 

11170. I took a larger similar cube of calcareous spar and put it up on the Torsion differential instrument (10925) with a piece of flint glass on the other side as a standard, and tried to observe position of the beam by reflection of a ray. When the Magnet force was on, the cube and the glass took up their relative positions according to their respective diamagnetic force. I could not find that there was any difference whether the cube had its Optic axis (always in the horizontal plane) either axial or equatorial, i.e. either perpendicular to or parallel to the Magnetic axis. But the cube was large, ½ inch in the side, and the arrangement imperfect.

11171. A group of Bismuth crystals which point well Magnecrystallically were then tried in a similar way, and being much smaller than the cube and of greater diamagnetic power, were better for the experiment. They were still opposed to the same heavy Glass, but when the Magnecrystallic axis was parallel to the Magnetic axis, were evidently nearer to the central line than when the M.C. axis was perpendicular to the Magnetic axis; i.e. they were less diamagnetic in the former position than in the latter. The group of crystals were turned round into diametral position with exactly the same result.

11172. This is as it ought to be. The bismuth conducts Magnetic force better in the direction of its M.C. Axis than in a direction perpendicular to it, and this is the cause of its set as a Magnecrystallic body.

11173. I filled a jar, glass, 15 inches deep and 4 inches in diameter, with a saturated solution of Proto sulphate of Iron, and then put it so that its upper and lower ends should act on a sensible magnetic needle suspended by cocoon silk as a bar of iron would act in the same position, i.e. provided the Earth's magnetic action is so far affected in its direction by the near vicinity of the solution. I observed the place of the needle by reflexion and it was sheltered from the air by a glass jar. I could perceive no sensible effect of the solution upon it.

11173½. Such a solution however is only 26 times stronger than oxygen (11067); and so a jar of oxygen 12 inches in diameter and 45 inches deep would be equal to it, and could hardly expect that this should be sensible. It would require a very sensible

instrument to shew its powers—or else one would expect greater effects by far in nature.

11174. Can the hot and deoxygenized air of towns have any sensible effect as it is carried off in different directions? Probably not enough in amount.

11175. Will a mass of heated air—heated by the Sun—tend to move outwards, i.e. upwards into places of weaker action. Gay Lussac and Biot say that intensity is less above, sensibly so. Now it is true the rate of decrease, etc. at the great Electro magnet may be ten million times greater than that at the earth's surface. But then $\frac{1}{3}$ of a cubic inch of oxygen there tends to go in with force of a tenth of a grain (11040). Suppose ten millions times that quantity tending outwards with one tenmillionth of the force, at the surface of the Earth. Then a cube of oxygen 200 inches in the side (or 17 feet about) would have twice that quantity, and $2\frac{1}{2}$ such cubes of air would contain as much oxygen at that, and the difference of temperature might be sensible. Besides, the assumption of ten million times the force at the average distance of an inch from the Magnetic axis is probably far too large.

11176. The Sun's warming effect will make the air expand and rise up in bulk above. What effect will that have?

11177. The barometer shews there is more air in weight above at one time than another. That ought to produce one of the irregular variations.

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11178. Suppose 2 wooden boxes, 18 or 20 feet long and 4 feet square, placed upright about 2 feet apart east and west—and a magnetometer placed between them. And suppose them made hot and cold *inside only* by steam thrown in or hot air, not burnt, isochronously with the vibrations of the instrument. Would it be affected by their alternate action? Cloath the boxes well to have heat only inside. I should like some expt. to shew a real effect of air.

11179. Try solution of Sul. Iron with a magnetometer.

11180. *Daily Variation* at Greenwich. Observation 1846, p. 93.

At 15^h 20^m. or 3h. 20. A.M. the N. end of needle is

		+22° 15′ 8″	101	West of the Astronomical meridian
17	20 or 5.20	−22 49 45	23	
1	20 P.M.	+22 54 50	335	
9	20 P.M.	−22 48 27	383 101	

The course of the N. end of the needle is something of this form*.

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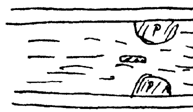
11181. For case of Magnetic conduction placed the small square glass cell ( ) between the flat poles for field of equal power. Put Saturated solution of proto sulphate of iron into the cell and arranged the torsion balance so as to allow any thing it carried to move equatorially. Hung two semi cylinders of phosphorous in the solution, and then a piece of bismuth, small, at the torsion balance to ascertain whether it would tend to go in between the phosphorous. If such an effect had taken place, it would have shewn that the lines of force were sensibly weakened there. The result was very doubtful. I could not sufficiently guard against currents in the air—or when the phosphorous was away, set of the bismuth in the one direction or the other by the magnet power alone (11168).

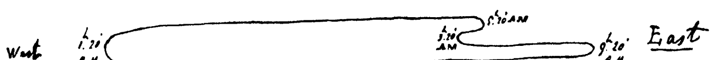


11182. Took out one of the phosphorous semicylinders and used a small globe of bismuth on the torsion balance; still the same uncertain result.

11183. Used a piece of Magnetic green glass in place of the bismuth—but the effect uncertain. The glass was probably not so magnetic as the solution.

11184. Cleared out the solution from the cell and filled it with water. Now repeated the experiment of Phosphorus repelling Phosphorus equatorially (11104). When for the fixed phosphorous I used a glass tube filled with a saturated solution of proto sulphate of iron, it attracted the moveable phosphorus. The effect was feeble but I am satisfied that it occurred. Thus in such a medium as water, a more diamagnetic body, as phosphorus, is repelled by other phosphorus, but a magnetic body attracts it. *They attract each other.*

* [11180]



11185. *Iron in a Torricellian Vacuum*—is affected by a small magnet outside just as well as if it were in air.

11186. *Remade* the former experiments upon the effect of heat on gases in making them more diamagnetic (9096, 7, 9269, 77), from a conviction that the presence of oxygen had lead in the case of air, etc. to a wrong conclusion. I used the apparatus as arranged (9231, 69), with the Vulcanized bottom for the shade to rest on. But to test the place of the hot ascending current, I took a thin plate of mica and covered it with white wax, and then placed this horizontally over the poles under the shade with the waxed side upwards.

11187. *Air* in the shade. When the helix was heated by 2 pair of Grove's plates to bright red, the hot stream rose vertically a little on one side of the magnetic axis and made itself visible above by melting the wax—beautiful indication. Cut off the current from helix and allowed the place to cool—its position was still apparent. Then made Magnet and then made helix hot. Now *no signs* of an ascending current of hot air, so far was it thrown aside from the axis and mixed up with the other air. So air as before (9096, 7).

11188. Attached a large Air holder of C. Acid to the pipe entering the French shade (it went to the top (9231)) and threw in about twice the capacity of the shade of C.A., which of course mingled with the air and gradually displaced the mixture. Hot helix without Magnet gave wax fusion just above. Hot helix with Magnet gave fusion of wax at a spot equatorial to the axis, and so shewed that the mixture in the Shade was affected by heat, though much less than air.

11189. Threw in more C.A. equal to twice capacity of the shade, and now the hot stream rose much nearer to the axial line than before—when Magnet on. Threw in two more volumes of C. Acid, so that very little air could remain, swept out, and now the hot stream of C.A. arose so perpendicular, notwithstanding the magnetic force, that the difference of place when the magnet was on and off could hardly be distinguished. So *Carbonic Acid* has not its diamagnetic power diminished sensibly by heat in this way (9230, 9269).

11190. Now adjusted another Air holder to the pipe and threw

in at once two volumes of *Nitrogen* made by burning Phosphorous in air. This being lighter than C.A. would displace it well and be pretty free from air. The hot current rose very nearly in the same place whether the Magnetic force was on or off. Its place was a little out when the force was on, but only a little and not more than was likely to be due to the oxygen in it. Nitrogen I think is unaffected by heat in relation to its diamagnetic quality (   ).

11191. Taking up the shade about an inch and instantly popping it down again, let in a little air to mix with the nitrogen. But now the hot stream was strongly diamagnetic compared to the colder parts. Letting in more air, this quality increased, and with all air the result was as at first (   ).

11192. So the effect of heat on common air is due to the effect on the oxygen. And as oxygen is Paramagnetic, so it is like Iron, Nickel and Cobalt in its power of loosing magnetic force by heat. What it might rise to by cooling, as in the North pole, we do not as yet know. It may be very great.

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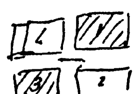
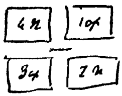
11193. Have had 6 Large Square bottles of flint glass made, 7 inches by 6 at the bottom and 7 inches high–well stoppered. Have cleaned them well out by Acid and then numbered them and their stoppers, 1, 2, 3, 4, 5, 6.

11194. Have hung up a small mirror with magnetic needles at the back, so as to point, the suspension being cocoon silk. Arranged to observe this by a telescope and scale reflected from the mirror at a distance of 9 feet, so as to have delicate indication. The mirror and magnets were in a close glass cell about $1\frac{1}{2}$ inches wide and upright. Observed the place of the reflector and its magnets under these circumstances and marked a zero ——.

11195. Then placed two of the bottles as in the plan thus. Thought they deflected the needle slightly thus , as if they were diamagnetic. But putting the other two bottles in place, the needle apparently took its first position. When bottles 1 and 3 were taken away, the needle seemed to set , as if the remaining bottles were diamagnetic.



11196. Filled bottles 1 and 3 with oxygen and Nos. 2 and 4 with nitrogen.



11197. Then arranged the bottles in various ways, so as to combine their effect in deflecting the lines of force. Then placing them as in the margin, I at first thought the needle set a little axially between the oxygens and equatorially as to the nitrogens. But by repeated experiments I could not establish this point. All was doubtful; the apparatus was very delicate and even the daily variation interfered.

11198. A bottle of solution of Sulphate of iron produced no effect. Neither did two bottles of crystals proto sulphate of iron produce any effect.

11199*. All the cases of deflexion of the terrestrial lines of force may be examined very well experimentally by a magnetic needle and the use either of two poles corellated, for the purpose of obtng. the active curved lines of force by which to curve or inflect the earth's lines—or better still by the use of a ring helix, which gives excellent curved lines of force acting at a considerable distance and that without the interference of local poles.

11200. Examd. certain of the principles laid down in my paper on the effect of declination, etc., and found it as expected. Good.

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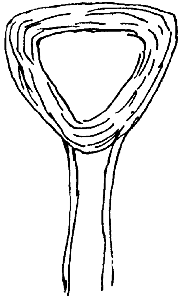
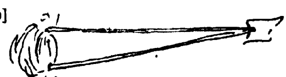
11201. Filled bottles No. 5 and 6 (11193) with saturated solution of Proto sulphate of Iron. Even these, combined in the most favourable way, did not affect the Magnetic needle, so that it was in vain to expect results from oxygen and nitrogen. (See 11249, etc.)

11202. The apparatus shews me the daily variation, but the effect on my scale is very small, so that it was not at all likely that a few inches or even feet of oxygen should shew any very sensible result.



11203. Have a ring helix of covered wire of this form and size about[1], containing about 25 convolutions of wire and 140 inches in length. It was arranged so as to be connected at pleasure with one pair of Grove's plates, and being black covering, one side of the ring, i.e. one face of it, was marked red with sealing wax

[1] The diagram is reduced to $\frac{3}{4}$ scale.

* [11199] 

to give position. It was so adjusted at the battery that the red face repelled the North end of a needle. When held therefore in the plane of the magnetic equator (of a dipping needle) with its red face upwards, it gave lines of force outside of its equatorial parts parallel to those of the earth and in the same direction of polarization.

11204*. So as regards these lines, they were contorted in the direction of a *diamagnetic conductor* under polarity, and a needle under the joint influence of them and the earth would shew direction of a hot globe of air, etc., provided it was not too near the ring helix; for if it were, it would come into the region where the lines of force of the helix return through the center, and that would give false result. About *a*, *b*, *c*, *d*, etc. the effect would be that of a diamagnetic atmospheric action; at *e*, *f*, *g*, *h*, *i*, it would be exaggerated greatly and still nearer, probably reversed. So keep the needle at such distance that the power of the earth is enough to right it.

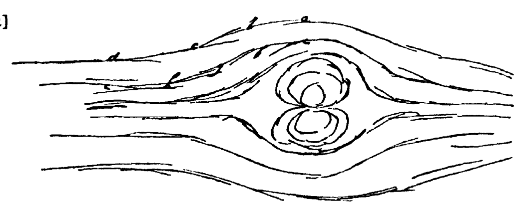
11205. Such a ring, if held in the equator of the needle, does not disturb its direction but attracts it bodily; and if moved right or left, the needle tends to place itself as a tangent to the thickness of the ring, i.e. in a plane perpendicular to the plane of the ring.

11206. When the ring was turned with the red side downwds., then also it affected the needle; it, if in the equator of the needle, repelled it bodily, not altering the direction. But when moved right and left, it altered the position of the needle in the opposite direction to what took place before, and therefore not as the line of air under the sun would do but as a cooled globe would act. So it repels the lines of the earth but it gives the reverse set as to position of the needle.

11207. The helix should be kept parallel to itself and to the plane of the dip; for if oblique, it acts on the forces of the earth otherwise than as a diamagnetic or paramagnetic globe would do. Strong effects are produced by twisting the helix a little, but I do not see that such twists can occur in nature.

11208. Set up a little horizontal needle on the table by cocoon silk and examined the effect of the helix red side upwards (   ) upon it. As it is difficult without a card to watch the gradual

* [11204]



changes of the needle as the helix passes in its neighbourhood, so it is better not to keep constant contact at the battery, but to put helix in place and then make contact, when the new direction of the needle is instantly seen however small. Or if not, by making and breaking contacts a few times, it can be made to swing and so manifest the action of the helix on it.

11209. Now when the helix was in *the Magnetic meridian*, all round the needle, above or below, or north or South, there was no change on the declination of the needle. There was of course much tendency to change in the dip in different parts of this circle, but not to affect a needle unable to shew dip.

11210. When the helix was in the *equator of the needle*, then also its position was not affected: for it is in such cases on the summit of the convexity formed in the lines of the earth, and being there, it is parallel to its position in the line if unaffected or in its normal position.

11211. In nature, the plane of the convexity must always be perpendicular to the lines of force. But then the whole system is not parallel to itself. Look at the two stations at St. Helena. They are affected through the great system above in the atmosphere and may perhaps go differently for the same great changes elsewhere, nearer to the sun when he is North.

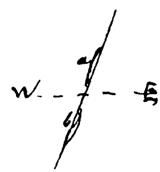
11212. Whether the helix was north or south of the needle, still, if it was on the same side of the equator and the same side of the mag. meridian, the direction of the declination added was the same. Thus, being on the East side of the needle, if between the Mag. meridian and the Mag. Equator, it was one way above the Equator and it was the other way below. Or if above the Equator always, then it was one way on the East of the meridian and the other way if the helix were west of the mag. meridian. So that the whole sphere may be considered as divided into 4 parts by one plane going through the mag. equator and the other through the meridian; in the two opposite quadrants it has one direction, and in the other two and intervening quadrants it has the other direction. This is for declination variation when the Mag. needle is not in the dip. If the needle were in the dip, then a simple law results, and it is the plane of the magnetic equator which divides into two hemispheres which would give contrary directions of

dip, and all would be resolved into dip in relation to the normal line of force.

11213. As on the earth the dip does not coincide with the direction of the horizontal needle, except in very few places and those not places of observation, so the reverse action on opposite sides of the Mag. meridian becomes *important*. Also, in places with very little dip, as St. Helena, the reverse action on opposite sides of the Magnetic equator probably *very important*; Cape of Good Hope, perhaps, i.e. as the place of greatest action, may be in the Sun or under the Sun however.

11214. The passing of the Magnetic equator is important, and the places where the Sun's path intersects it will vary considerably as the Sun changes between the tropics and as the declination is different and as the dip varies. Must consider all these, for when the intersection is above the horizon, and at different distances on the opposite sides, it will make much difference in the daily variation.

11215. The effect of an inclined Mag. meridian, as regards the sun's path, is the effect of passing the meridian above referred to ( ), and the expt. sustains the former conclusion drawn in paper. Observe that if a needle is on the meridian north or south of the sun's path, then the action is much stronger on it on that side of the meridian which makes an acute angle with the sun's path. Thus it will be stronger on *a* in going from E to W, before it has passed the meridian; and stronger on *b* after it has passed. Which is natural enough–but makes part of the variation in natural case.



11216. Have marked on the table a line for the Magnetic meridian –have placed a wire at the end of it in the position of the dip– have fixed a plane beginning at the bottom of the dip and inclined so as to be at right angles to the dip and represent the Magnetic equator plane. Have suspended a small needle at right angles to the dip by cocoon silk so as to approach very nearly to the plane, and have so the means of marking out the Magnetic meridian line on the plane and also magnetic East and west, etc. etc.

11217. Used the helix as before–red side repels the North end of the needle. When the red side up, then a needle on the outside and in the plane of the helix would stand as a dipping needle does

by the action of the earth, and on either side of the plane of the helix it would be deflected just as if the helix were the center of a diamagnetic or hot ball of air. The helix was preserved constantly parallel to itself and to this equatorial plane.

11218. The helix in the plane of the Magnetic equator passing through the centre of the needle—*no declination* produced by it.

11219. The helix in the plane of the Magnetic meridian passing through the needle. *No declination.* These are the only positions in which it produces *no effect.*

11220. In any other position, the helix affects the needle. So there are four quadrants in which it is thus active. And whenever the helix passes the plane of the meridian, the direction in which it affects the needle is changed; and whenever it passes the plane of the Equator, the direction is changed.

* 11221. The quadrants are alternate. Thus if the helix be carried round the needle in a circle in a third plane passing through the needle, at right angles to the two former, and beginning at the upper meridian, there it will have no action. As it goes west and sinks, it will deflect the needle in one direction more and more until about 45° on its course, when the power will diminish and become nul at 90° or the place of the equatorial plane. Passing that, it will deflect the needle in the reverse order to the former change, and this will increase and then diminish until at the 180° or lower part of the Mag. meridian it will cease altogether. Then it will acquire power to deflect the needle as at first, until it reaches the Eastern side in the equatorial plane, where being nothing, it will as it rises and goes west to the upper meridian receive power to cause deflection of the second kind, and then that will pass away. So that whilst it goes round the needle, the deflection is produced four times, alternately in opposite directions.

11222. If one were to imagine a sun going round the needle and able to act on it the whole time, as the helix does, and if we began with the needle in the middle of one of the quadrants, so as to be at extreme west for instance, then we should have two alternation[s] thus, from extreme West to extreme East, and then to extreme West and finally to extreme East, ending with extreme West by the return to the first place. In that double oscillation, four zero points would have been passed.

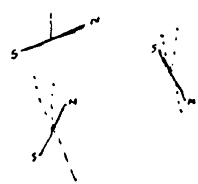
11223. This double oscillation is represented to us in nature by the day and night action, but exceedingly distorted by the conditions under which the air takes up and represents the action of the Sun.

11224*. If the helix be in a certain magnetic latitude to the needle, and then be moved along that latitude from one magnetic meridian to the other, it does not change in the direction of the declination produced by it though it does in the amount. Thus in the figure, in which the needle is at the center *a*, the helix being always parallel to the plane *b, c, d, e,* if carried round that circle, produces no effect. Or if carried round the meridian circle (in London) *b, f, d, c*[1], produces no effect. If it be at *h*, it also produces no effect, but if then carried round by *i* to *k*—then it has power to produce the same declination in all parts of that course, greatest at *i* and diminishing to nothing as it goes either way to *h* or *k*. Diminishing also to nothing as it goes either way from *i* to *g* or *c*, and so for all the other quadrants.

11225. So the helix may be astronomically either north or South of *a*, and yet the declination not change in direction. If there were great declination originally, then it might be astronomically even East or west of it and yet not change in character. But it cannot be Magnetically North or South of it, or East or West of it, and not change in declination.

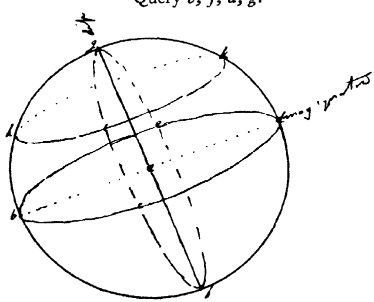
11226. The great circles of no action very important in Atmospheric Magnetism.

11227. The needle used was slung so as to rest in the Magnetic equator. Slung two others, one very nearly in the line of the dip so as to point thus, and the other very nearly vertical, to point inclined to the dip in the opposite direction. Employed all these in succession, but they were affected exactly alike and shewed no difference; i.e. the South end always went the same way for the same position of the helix. If the helix was very near, then one pole was a little more influenced in certain positions than the other, but distance removed that difference (easy to be accounted



[1] Query *b, f, d, g.*

* [11224]



for) and gave pure results. The helix being above or below the line of the needle prolonged, made no difference, provided it was in the same place as regarded the magnetic equator of the earth's lines of force passing through the needle.

11228*. The helix should be so placed that a magnetic needle freely suspended in the plane of its equator and external to it should point by its force as a needle points by the magnetic force of the earth. Then the disposition of the lines about its equator represent the bulge in the earth's lines by a diamagnetic globe, in direction at least, and above or below it, if at a distance. At such times the needle is, as a whole, attracted when the helix is in its magnetic equator, for then the two poles of the needle both tend to turn in and pass round through the axis of the helix. The helix must be kept so far off from the needle as to be clear of this inversion, or even the exaggeration of its position.

11229. Now reversed the helix so as to have the black side upwards. Now a needle placed within its axis would have the position of one in the dip, and outside it would tend to have the reverse position so†: and the lines in the air are drawn in by the reverse lines of the helix at the middle parts, as *a*, *b*, so producing the form of the lines in a paramagnetic conductor.

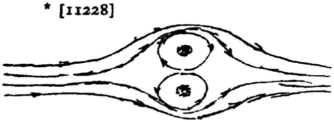
11230. This helix, either in the plane of the Equator (magnetic) or in the plane of the Mag. Meridian, produced *no declination*. It produced declination in the four quadrants, as the former helix did, but always in the reverse direction. Hence the passages from one direction to the other on crossing the lines of no declination occurred exactly as in the former case.

11231. Employed a round ring helix in place of the former (   ); the results were exactly the same.

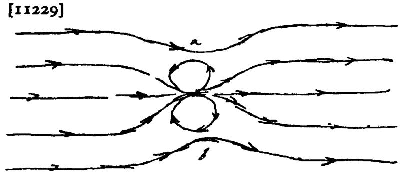
11232. Make a model of a magnetic globe, its quadrants etc., for as the action is greatest in the middle of the quadrant, it is important to see how that quadrant is placed in relation to the Sun's passage, and where the planes of no declination are and when he crosses them. For as the places of force altogether depend on the Magnetic equator, dip, etc. etc.

11233. St. Helena, dip 22°–Declination 23° or 24° West. I think I see the cause of the reversed action in June, July, Aug., etc. in the position of the Magnetic equator and meridian. Compare

* [11228]



† [11229]



St. Helena with Hobarton in this respect. There, Hobarton has Dip 70° 39′ S. Declination 9° 60′ 8″ East.

23 SEPTR. 1850.

11234. Have mounted a small dipping needle on an horizontal axis of cocoon silk. It can move only in one plane and that at present is the magnetic meridian of London. Have also the black helix as before and shall use it with a single pair of Grove's plates, keeping the red side upwards and the helix plane parallel always to the magnetic equator, so that it shall give the form of lines due to a globe of hot air or the sun acting on the atmosphere. The dipping needle in its natural position then stands parallel to the axis of the ring helix but as a tangent across its outside curvature (   ).

11235. There is no deflecting action on the needle whilst the helix is in the Magnetic equator.

11236. No deflecting action on the needle whilst the helix is in a plane vertical to the equator and passing through the axis of the needle, i.e. in a plane vertical to the plane of motion and to the magnetic equator.

11237. These planes divide the space around into four quadrants as before and the needle, being in the mean position, is moved in the one or other direction alternately as the helix passes through these quadrants.

11238. If the helix is in the upper North quadrant, the upper or south end of the needle goes south—if helix in upper south quarter—upper end of needle goes north—if helix in lower south quarter, the upper end of the needle goes south, and if helix be in lower north quadrant, upper end of needle goes north.

11239. So as the helix travels round the needle through these quadrants, if the needle be first in a mean position, its end goes north until in place of maximum action of the first quadrant, then south past the neutral point until helix in place of second maximum action—then North again until helix gains third maximum place—and then South until helix in fourth maximum place, and then North until both helix in neutral place and needle in mean position.

11240. The simple fact is that, if the helix be below the equator,

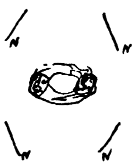
then the lower end of the needle tends to point outwards from it or outside of it, all the way from the equator north to the equator south, the under needle pole being *repelled* by the axis of the helix but *drawn* by the outer lines. Or, if the helix be above the equator—then the upper end of the needle does the same thing in relation to the under side of the helix.

11241. The needle tends continually to place itself as a tangent to the wire of the ring helix or the currents carried by it.

11242. I turned the needle support round 90°, so that now the plane of motion, being vertical, was East and West instead of North and South. The plane of no disturbance was also turned 90° being now North and South. It is simply the plane in which the needle is restrained mechanically by the mode of suspension from obeying the impulse which the helix exerts upon it.

11243. If the needle were free to move in every direction except to leave its place, then the Equatorial plane would be the only plane in which the helix could be without affecting its position—a line forming the axis of the plane being also a place of no deflexion but giving positions of instable equilibrium.

11244. Then if the helix were carried round the needle in any circle of latitude, the needle would revolve, its ends describing circles, and its two halves cones—the end nearest the helix being held out from it as if repelled. So if there were one helix in the middle and two dipping needles, one above and one below, were carried round in circles of latitude (the helix being then in the plane of the equator), their ends nearest the helix would be sent outward and those halves describe two cones. They are exactly the two cones of the diamagnetic conductor or globe of hot air and needle in it (   ).



11245. Here in dip as before in declination (   ), it is not the direction in which the needle stands, for it may be leaded or otherwise affected as all horizontal needles are, but it is the *direction* of the lines of force at the needle that govern all. The helix may be above or below the prolongation of the needle indifferently, for if it still continues on the same side of the line of force, then the end of the needle moves in the same direction, though it may [be] towards the helix at one moment and from it at another.

11246. As an expression of the facts for use in applying them to natural phenomena. When the helix (or air sun heated) is above the needle, it sends the upper end from it. If helix north, the end goes South; if helix south, the end goes north; if east, the end goes west, etc. As in the declination case, it seems to repel the near end of the line of force. All very consistent.

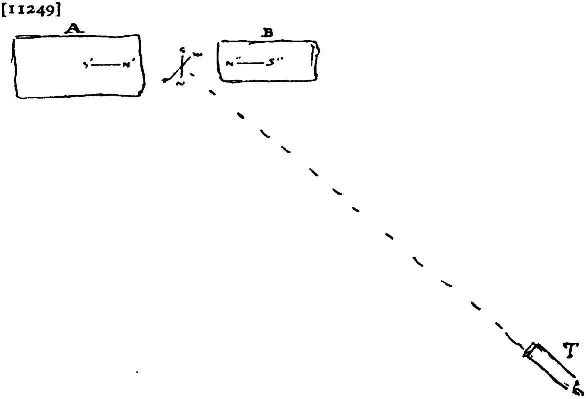
11247. As an expression for the effect on the horizontal needle. If the helix (or hot air) be above the needle and therefore above the Magnetic equator, the Helix on the East of a needle having North dip sends the south end West; and on the East of a needle having South dip, sends the North end West. As with the dip, it seems to repel the end of the line of force nearest to it (   ).

14 OCTR. 1850.

11248. Suspended a short needle over the middle of a long bar magnet, and over the poles of a weak horseshoe, with iron, etc. underneath and about it in various position[s], to see if the general action of the helix was the same, or whether it produced an inverted action as at St. Helena; but I could not find any difference in the direction of the motion of the needle: that was the same as before, though the amount, etc. was often altered.

11249*. With a view to procuring an experimental effect of oxygen on a magnetic needle or on the lines of force governing it, made the following experiments with a saturated solution of proto sulphate of iron—to repeat them if successful in the same form afterwards with oxygen in place of the sulphate. *m r* is a small mirror, not quite an inch square, hung by a single fibre of cocoon silk so as to be in a vertical plane; S, N represent two small magnetic needles about ¾ of an inch long, each attached to a piece of tin foil itself attached to the mirror, so that the needles hung below the mirror and when in their natural position being oblique to it. Placed the mirror as represented, with its face towards the telescope and scale at T, placed 126 inches distant. A and B were two glass pneumatic troughs, about 12 inches long, 5 or 6 inches wide and 4 or 5 inches deep, placed as near the magnet mirror (which was in a glass case) as could be so as not to interfere with the observations at the telescope. S′ N′ and N″ S″ are two large sewing needle[s], about 2½ inches long, well magnetized and fixed on leaden supports, so that when placed in the baths A and B, they should be about 1¾ inches from the bottom and in a line with each other. Either one, in the place represented, would deflect the mirror magnets much, sending the N end away; but both were so adjusted as to have equal and contrary power, so that S N held its natural position. The observation of the position of the needle was made as in the case of Magnetometers, by the eye at the telescope T looking into the mirror at the magnet, which reflected a scal[e] of lines placed at the object end of the telescope (   ) as usual. At this distance of 126 inches, a degree would occupy 2·2 inches, but as the

* [11249]



motion of the mirror doubles the effect, so a motion of it through 1° would equal 4·4 inches on the scale. Though my scale was rough, still I could easily distinguish difference of $\frac{1}{20}$ of an inch, which was less than a minute of a degree. (See 11193, etc.)

11250. Adjusted the scale so that the wire in the telescope and a standard line in the scale coincided. Then filled A with water and B with a solution nearly saturated of proto sulphate of iron, so that both the lateral magnets were in the middle of the fluid. Their ends N′ and N″ were about 3 inches from the reflector magnets and about $1\frac{1}{2}$ inches within the extent of the liquids towards the mirror. There was a little disturbance of the mirror, but when it had settled, it was as before.

11251. Drew off by a syphon the solution of iron from vessel B, the standard line of scale appeared to move a little to the right of the wire in the telescope. (The telescope inverts the image.)

11252. Drew off the water from A, which appeared to make the standard line go to the left of the micrometer wire–instead of making it go more to the right. Replaced the water in A, but the mirror kept its last place. Some little touch or tap perhaps produced the former effect. All has been carefully fixed and is on a stone floor. Still, the passing of a carriage make the mirror move and swing a little.

11253. Water in A. Air in B. Made standard line and micrometer wire coincide–took out the water. No difference.

11254. Put sol. Sul. Iron into A. Air in B. The standard line is a little to the left of the wire in the telescope. Put water also into B–and now the mark was a little more to the left in the telescope.

11255. Took both sol. iron and water away–still the mark a little to the left of the micrometer wire. Placed the sol. of Sul. Iron in B; still the mark where it was before–no sensible effect of the iron. Placed water in A, which seemed to send the mark a very little back to the right towards micrometer mark. There was very little effect indeed, if any–and if any it might be due to temperature.

11256. When the mark goes to the left *in the telescope*, it is as if the magnet in A were weaker in power than before, or the magnet in B stronger; and when it goes to the right, it is as if the magnet in B were weaker and that in A stronger. This I verified by trial.

11257. There was no appearance here of any effect of the solution of the proto sulphate of iron and of water, either together or

separate, over the direction or power or condition of the lines of magnetic force of the two small magnets in A and B.

11258*. Removed the vessels and magnets A and B and employed two strange coarse magnetic needles about 9 inches long and $\frac{1}{2}$ or $\frac{3}{4}$ of an inch broad, being made from plates of steel. Placed these on the table about 10 and 11 inches off from the mirror on opposite sides, as in the former case, so as to neutralize each others effects, and then placed between them and the mirror magnets one of the large square bottles filled with proto sulphate of iron solution (11201). But whether one bottle was at A and air at B, or one at B and Air at A, or whatever the arrangement, I could not perceive that the solution of sulphate of iron made any difference.

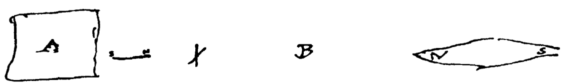
11259†. Now put a small needle about 3 inches from the mirror magnets—and a large magnet about 11 inches off to balance it in power. But whether the bottle of solution of sulphate of iron was at A or B—or in any other position, it apparently produced not the slightest effect on the influence of either the small or the large needle on the mirror magnets.

15 JANY. 1851.

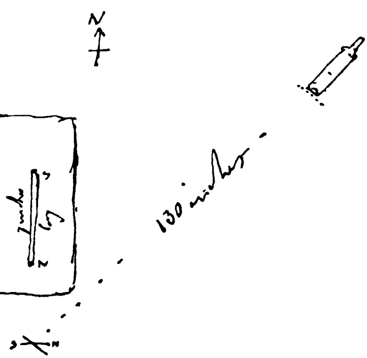
11260. Arranged an apparatus thus‡: the mirror magnets at S N to the south of a glass trough 12 × 5 × 5 inches. In it a magnet

* [11258]

† [11259]



‡ [11260]



about 7 inches long fixed so as to be 2 inches from the bottom and about 5 inches from the magnet mirror, and with N end towds. the latter so as to deflect it into a position nearly at right angles with the Magnetic meridian. The observing telescope was about 130 inches off. All was adjusted until the central thick standard line of the scale appeared behind the micrometer wire.

11261. The magnet had been just before excited fully and so was perhaps not very permanent in state. A carriage passing by set the mirror vibrating, although all stood on a stone floor.

11262. Mark and wire were made coincident. Then sol. proto sulphate of iron put into the trough up to level of the magnet: mark set to the left of the wire (in the telescope). Trough filled up with solution; now the mark had gone to the right and was in its first place.

11263. Things outside apparently disturb matters much—or else internal changes come on irregularly in this new magnet. Whilst looking at the mirror, vibrations came on (no carriage going by), which increased until in 3 or 4 swings the extreme edge[s] of the scale card appeared alternately passing the micrometer wire; and then they diminished again—and a cab coming by, they suddenly stopped and the mark was at zero or the micrometer wire. It simulated very much an Aurora action.

11264. Mark being at zero, took out the solution and now the mark was to the right hand of wire. Put in the solution, but the mark more to the right than before. In fact the magnet in the trough is loosing power and that irregularly, and I have to bring the telescope more and more round towds. N. of the magnet mirror to keep the scale in sight.

11265. As the solution is slightly acid to keep all peroxide in solution, I thought that chemical action on the magnet might join in this effect. Readjusted the position, and now as the scale mark seems very steady, took out the solution; but mark had gone to right whilst doing so—put in the solution, has gone a little more to the right. Took out the solution—the mark went a little to the left, as if power had increased.

11266. Now prepared some dilute Sulphuric acid, able to act slightly on iron or steel. Mark and wire coincide—put in the acid—the mark went considerably to the left as if magnet .

Left it with the acid in at 6 o'clk. (the mark having returned to the wire). At 7 o'clk. the mark was to left of the wire. Chemical action is going on, for hydrogen is slowly escaping and films of carbon are set free on the magnet. Took out the acid and the mark went well to the right. Put in the acid, the mark went a little to the left—taking out the acid made it go to the right. Put in the Sol. Sul. Iron—mark went *more* to the right. Took out solution —mark remains on the right. Put in water—this made the residue of iron solution turbid but threw mark much to the *left*. The effect is clear, but is it not all due to temperature of the magnet?

11267. Took out the water—the mark is to the left; but all being left, it gradually returned and even went to the right of the micrometer wire. The temperature of the Sol. Sul. iron was 55°·5F.

dilute acid „ 54°
water last used „ 50°

11268. The mark being to the right of the wire; water at 62° was put into the trough—the mark and card image was all away to the right. So much effect had raised temperature in sending it to the right, as a lower temperature before sent it to the left. Took out the water; the mirror needles gradually and slowly returned to left as the magnet cooled by the air from 62° or 61°.

11269. Ether dropped on the bar quickly brought the needle back, sending the mark to the left. A little warm water dropped on the magnet sent the mark enormously to the right—diminishing the power of the magnet. Ether brought it partly back—but not nearly all the way.

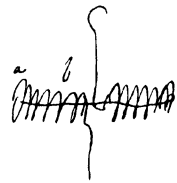
11270. The Magnet seems to lose power by all these changes. May be able in some degree thus to examine its internal condition.

11271. From all these results, I conclude that under this form of experiment, neither Water nor a solution nearly saturated of proto sulphate of iron—nor air—being employed as media round the magnet in the trough—produced any sensible difference in the distribution of its power, and therefore that oxygen gas would not do so. The apparatus being sensible to minutes of a degree.

11272. Hence there is probably no reason to expect any change in the intensity of the force of magnetism at any given station dependant on the direct action of the hotter or colder atmosphere upon the needle at that place—but only by its effect in disturbing the great distribution of the *whole* of the force around the Globe.

11273. Have been endeavouring again (from MM. Schlagintweit's instruction) to procure Du Bois-Reymond's effect of Muscular Electricity from the arms, but cannot succeed. I obtain plenty of current. If agitation in one finger basin by motion of finger is greater than in the other, then there are regular currents; or if one finger in and out more or less, always preserving the circuit, then consistent effects; but account being taken of these, I can obtain none for muscular contraction. I suppose it is because I do not know how to proceed; still, I do not see any cause for failure if the effect be real.

11274. Have been experimenting with the three helices sent me by Dr. Greifswald[1] (See Phil. Mag., Jany. 1851). I find a difference, it is true, but not in character. When they are up, a hand helix held end on affects the two ends of the helices on each half of each of the three apparatus, i.e. at *a* and *b*, differently; attracting at one place and repelling at the other. If with the helix of uniform current, though the attraction be at *a* and the repulsion at *b*—then with the helix having more current at *a* than at *b*, the repulsion at *b* is less than for the standard helix; or with that having less current at *a* than at *b*, the attraction at *a* is less than for the standard. A natural consequence of the position of the mean for all the currents being moved right or left of the distance between *a* and *b* by the mode of coiling. I do not see any thing more—nor any repulsion *end on* for one and attraction for the other.



1851 MAR. 18.

11275. I have the differential balance from Newman's and think it will do—as also the irons: the double cone arrangement new, etc. etc. I have been making some cradles of copper wire to carry the cylinders, etc. of different substances and also various counterpoises, etc. etc. The weights of these and of parts of the balance are as follows:

a.	The copper beam with its two sliding pieces . . .	802 grains
b.	The Slender Platina wire cradle	40
c.	The old or first copper wire cradle (narrow) . . .	96

[1] The reference is to Dr. von Feilitzsch, of Greifswald in Prussia, whose letter Faraday communicated to the *Philosophical Magazine*, vol. 1, 4th Series (1851), p. 46.

d.	The new copper cradle (narrow)	88 grains
e.	The new wide copper cradle	91
f.	The copper cylinder for Gas (without stoppers) . .	310
g.	The brass Do. . . . . Do.	232
h.	A Glass gas cylinder (such as those used, 10968) . .	45

11276. Wire counterpoises—of *copper.*

Thick copper wire No.	grains	Thinner copper wire	grains
1	824 grains	1	68 grains
2	480	2	67
3	337	3	57
4	378	4	51
5	296	5	45
6	215	6	35
		7	37
		8	23
		9	23

1851 JUNE 21.

11277. At present the point of suspension is 4·75 inches from the cross bar carrying the bodies to be experimented with and only 3 inches from the place of the counterpoises in the opposite direction; so that the weights respectively applied must be as $\frac{3}{4{\cdot}75}$ after the beam is balanced.

11278. The cradle = 88 gr. *d* (11275) is well counterpoised by thick wire weight ii or 480 gr.

11279. When the glass tubes 1 and 5 (10969) were in the cradle, then the addition of thick wire weight iiiiii = 215 gr. inside the other, balanced it sufficiently.

11280. The bismuth cube $\frac{1}{2}$ inch in the side without the cradle was balanced by thick wire weights ii and iiiiii to counterpoise it with the small wire weight iiiiiii on the arm on the experimental side.

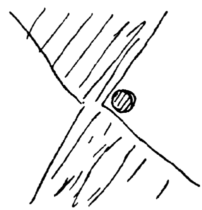
11281. The little bismuth sphere ($\frac{1}{4}$ inch diameter about) requires the thick wire weight iii to counterpoise it and the beam.

11282. When the beam alone is balanced, suspended by the platina wire before described (11108) (whose length is 22 inches nearly and diameter   only, its weight being 4 grains for 114 inches), it vibrates very slowly over the medium or mean line of the magnetic poles, making largish excursions on both sides of it, as if it were repelled in consequence of its diamagnetic character. It seems almost inclined to come to rest in an oblique

position on either side of the medium line, and indeed this should be so. This need not interfere with the accuracy of the instrument when the beam is always brought back by torsion to a given position.

11283. As the magnet is not at present excited (the great horse shoe Electromagnet) but only in a residual state, it indicates very great sensibility of the instrument with this wire of suspension and weight on it.

11284. The beam alone being in the medium line, then hung on a small bismuth ball about 0·25 of an inch in diameter by thin copper wire, so that the ball was in the horizontal plane passing through the center of the magnetic field. The magnet was unexcited and only in the residual state, but the ball was repelled away and went off until the beam made an angle of 9° or 10° with the medium line. On giving 40° of torsion, it was not enough to compensate the effect, for the ball did not return more than 8° or 9°, leaving one degree uncompensated for. This shews the great diamagnetic power of bismuth as shewn by this balance instrument.



11285. Sent a current from 2 pr. of Grove's plates through the magnet and now the repulsion of the bismuth ball much greater. When 360° of torsion were put on, still the bismuth ball was 7° or 8° from the normal place, and to overcome this it would have required many times this torsion, and such a force as would set the wire.

11286. This wire is not thick enough for such cases—for when a length of 8 inches is submitted to a torsion of 300°, the wire takes a set; a length of 12 inches ought not to be tried by more than 200° of torsion.

11287. Took off the bismuth ball and put cradle *d* (11275) on the arm; put tubes 5 and 6 (10969) and counterbalanced them—appeared very nearly equal. With current of 2 pr. Grove's through the magnet, No. 5 seemed as if it tended to go out. These two are Nitrogens 1 *at.* and ½ *at.*, and they should be alike—the apparent difference may have been in the amount of diamagnetic solid matter.

11288. Tubes 1 *Ox.* 1 *a.* and 6 *Nitr.* ½ *a.* (10969) with current of 2 pr. of plates. Tube 1 was most attracted. But what with the

weight of the beam and probably the diamagnetic action of the cradle (which is the narrow one), the motion is very slow and the apparent effect small.

11289. Tubes 2 *Ox.* $\frac{1}{2}a$ and 6 *Nit.* $\frac{1}{2}a$. were put in—the difference was still smaller.

11290. Must dispense with the cradle and try the tube alone as before (11314).

25 JUNE 1851.

11291*. When the beam alone was balanced and the line of rest adjusted to the mean line, and then the balance taken away from the magnet, and the vibrations of the beam by the torsion tried, a to and fro vibration occupied about $3\frac{1}{2}$ minutes. When the balance was put in place over the magnet in its residual state, the vibrations appeared to be a very little slower.

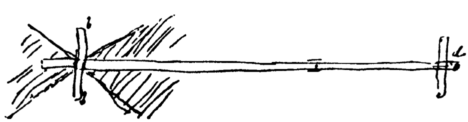
11292. When the current of 20 pr. Grove's plates was sent through the magnet, the beam vibrated much quicker than if there was no current, and tended to settle in the mean line as if *attracted*. It was clearly as if attracted, and considering the tools that have been employed in making this copper beam, that is no wonder. But the amount of attractive force may require to be estimated (11318).

11293. Prepared tubes with thread loops, so as to hang them from the cross bar, *b b* (11291), equatorially over the magnetic field; when equipoised by wire weights at *d*, the same horizontal plane bisected the tubes and also the iron core at Magnetic field. Two tubes, namely No. 1 *Oxy.* 1*a.* and 5 *Nit.* 1*a.* (10969) were hung on at divisions 6 from the beam on each side (which divisions are about $1\frac{3}{8}$ inch apart). Then the current of either 2 pr. of plates or 20 pr. sent the oxygen tube inwards and the nitrogen outwards very well.

11294. Put on a torsion of 360° so as to hold the Nitrogen inwards against the iron core. Then the current of the 2 pair drew up the oxygen and sent off the nitrogen till they were about equidistant. The 20 pair also did the same thing, but much more powerfully and quickly. The distances in the two cases seemed nearly the same, but I did not examine that point.

11295. When the torsion was 720°, the two pair current affected

* [11291]

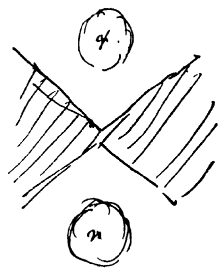


the bulbs, separating the nitrogen from contact with the iron and bringing in the oxygen. The 20 pr. did the same thing far more strongly.

11296. Hence there is plenty of sensibility—in fact, the wire now in (11282) is not strong enough and will not bear the needful torsion without setting.

11297. When the torsion was 360° and the 20 pr. of plates in use, the oxygen bulb did not find its place of rest in contact with the core. When the 720° torsion was on, its place of rest was certainly at a little distance from the core, but not far off. What a single bulb of oxygen will do (not antagonized by a bulb of nitrogen on the other side) we shall see presently (11304). The glass of the bulbs goes for much here.

11298. Removed the bulbs farther apart to the divisions 9, 9, which are about 2 inches asunder: they are now farther out in the magnetic field and farther off from the core. With no torsion on the beam and a current of 20 pr. of plates, the oxygen was drawn up strongly, driving out the Nitrogen—the effect was much finer than before (11293, 4) with the smaller distance. Its place of rest was clearly away from the core and *farther off than before*; for now the diamagnetism of its glass is not counteracted to the same extent as before by the diamagnetism of the Nitrogen glass, the latter being farther away.



11299. It would appear that at a certain distance the Magnetism of the oxygen and the diamagnetism of the glass neutralize or compensate each other; for there is no sensible torsion on now, or only 2° or 3°. Probably this may serve as the means of obtaining a standard of magnetic or diamagnetic force, that of the glass alone always being ascertained.

11300. The 2 pr. of Grove's plates shew the same effect but less powerfully and more slowly. The resting place of the oxygen bulb seems as if it might be the same as before (11299); and probably so it ought to be, for the action on both the gases and both the tubes would be in the same proportion, there being no torsion on the wire. Can not this be employed to set me free from the varying power of the magnet?

11301. A torsion of 720° put on in favour of the Nitrogen made the resting places of the two bubbles of *Ox.* and *N.* nearly

equidistant when the current was on, i.e. the beam was nearly in the normal position. As the torsion is lessened, the oxygen has its place of rest nearer to the normal line up to a certain distance, but as said before (11298), when the torsion is nothing, it is still held at a considerable distance from that line by an action of the glass.

11302. The suspension wire is too fine for these forces.

11303. Perhaps the plan will be to block up the oxygen, so as to make it stand equidistant with the nitrogen from the neutral line, and then to put on torsion force just enough to keep it at that distance when battery power is on, varying the torsion force until it just relieves; i.e. if I cannot contrive a damper which shall admit the bubble and beam to go into their place at once without oscillation–then the blocking up could be dispensed with.

11304. Removed the Nitrogen bulb and attached the *oxygen bulb No.* 1 (10969) *alone* to the arm at mark 1 on the cross bar. So considering that the beam had no torsion, but tended to stand in the mean line, and that the diameter of the glass bulb was about 0·5 or 0·6 of an inch, so there must have been about 2° of torsion tending to hold the oxygen close to the core. Now when the oxygen was swung off into the distance, the current from 20 pr. of plates pulled it well inwards, but *not into contact.* It has its place of rest away from the core, and the effects of this kind are as good as if the nitrogen were on the beam, but are now more simple, as the oxygen and its glass bulb shews all the effects.

11305. The drawing in of the oxygen by the magnet is better shewn when it is 35° or 40° off (by the beam scale) than when it is closer in–the action is quicker and more powerful. At the same time, the diamagnetic power of the bulb containing it is less, because the difference in distance of its acting mass and that of the oxygen is less. Perhaps one may use this principle, comparing bulbs of Ox. and Nitrogen at *large distances* from the axis of power.

11306. In order to obtain the diamagnetic condition of the *glass bulb*, I removed the oxygen and hung in its place a *glass bulb* filled with air and open at the lower extremity–the bulb was of flint glass. *This bulb has a place of rest* with 20 pr. of plates current on the magnet. If it is beyond the place of rest, the magnet *draws*

it inwards: if it is within the place of rest, the magnet *drives it outwards*. The place of rest is farther off than with oxygen bulb, for then the attraction of the oxygen draws it inwards. It is with 20 pr. of plates about 8° of the beam away from medium line, and then a torsion of 8° is tending to carry it inwards. How can all this be with glass?

11307. There was a counterbalancing wire on this part of the beam. Thought it might do something so took it off, but the effect was still the same.

11308*. Now removed the bubble and used a solid flint glass cylinder which was hung in the same place. It was just like the glass vessel; *within* a certain distance it was repelled outwards; without a certain distance it was *attracted* inwards—very clearly and distinctly.

11309. In order to know the relative value of glass and metal vessels for holding the gases, I rolled up certain tubes from platina, silver and copper sheet, cleaned well upon the surface, making them represent cylinder vessels. The glass cylinder was removed and now the *copper tube* hung in its place; its absolute weight was 70 grains. There was no torsion on the beam; length of tube 1·7 inches, diameter 0·75 inches.

11310. When the copper was near and the 20 pr. of plates put on and the *contact preserved*, so that magnetic force was continuous: the copper was at first powerfully repelled. When it had gone to some distance, the repulsion gradually and quickly *ceased* and the copper slowly returned as if *attracted*, being evidently held by the magnetic forces, and at last it came into close contact with the iron and I think *stops* there, as if permanently *attracted* and not repelled diamagnetically (11319).

11311. Repeated the result—again the same phenomena—being at first repelled, it stops, and then returns; quickly at first, then slower and slower, and at last approaches so slowly and regularly into contact with the core, that I think it is clear it waits for or upon some change which time is effecting either in it or in the magnet (11319).

11312. When electric contact is made and broken quickly, there is of course strong repulsion and as strong attraction, due to the induced and contrary currents produced at the moment in the copper.

* [11308] 

11313. When Electric contact is made and continued, and whilst the copper is approaching to the magnet, [if] it be carried off by the hand, it moves as if in a thick fluid, being held by the currents which the motion induces as formerly described. Still, if left free at any moment, it returns slowly towards the magnet as if it were permanently attracted–slowly only, but still as if it were *attracted*. It is perhaps magnetic and this may cause the final attraction, the induced currents producing the first effect. *I think copper will not do for the gas vessels.*

11314. This is probably the reason also why the copper cradles interfered with the motion of the glass bulbs (11288, 90) of oxygen and nitrogen.

11315. Took down the copper cylinder or tube (11309) and hung in its place a small solid copper cube about 0·3 of an inch in the side, so as to obtain a purer and more concentrated result. When the magnet was excited, the cube was first repelled and then attracted, but it did not stand in *contact* with the iron core. If moved out, it went in again slowly, finding a place of rest a little distance off. It was *attracted* if beyond that distance and *repelled* if nearer; so it is like the copper tube. (11319.)

11316. The *silver plate tube* (11309) weight 46 gr., length 2·2 inches, diameter 0·5, was now hung up in the place of the copper tube. It moved outwards and then came in again, just like the copper, by induced action; but it did not go quite up to the core. It seemed rather indifferent as to magnetic or diamagnetic action. When it was situated up in the angle, it was striking to see with what difficulty it moved, because of its good conducting power and consequent induced currents. It will not do for Gas vessels if a changeable magnet is used, but may (as also copper) serve well with a permanent magnet, if it has no special mag. or dia-magnetic relation (11319).

11317. *Platina foil tube.* Magnetic at once and directly attracted. It gave no signs of the first repulsion and following attraction (11310, 16). Weight 45 gr., length 1·4 inches, diameter 0·4.

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11318. Tried the beam alone without any charge of tubes or other matter. It was attracted (20 pr. of plates used) into the normal

or mean position pretty strongly–probably because of the iron derived from the tools used in making it. Put on 40° of torsion, blocking the beam at 15° of distance from the mean line; then the magnet, being excited, easily pulled it in towds. the mean line. Put on in succession 180°, 360° and even 720° of torsion, and the magnet was still able to pull the beam away from the block at 15°.

11319. It was this attractive force which brought up the silver, copper, glass, etc. (11310, 1, 5, 6) to a certain distance, and not any peculiar power in them to find a peculiar place of rest. Must have another beam–perhaps a quill glass tube and also a much thicker wire.

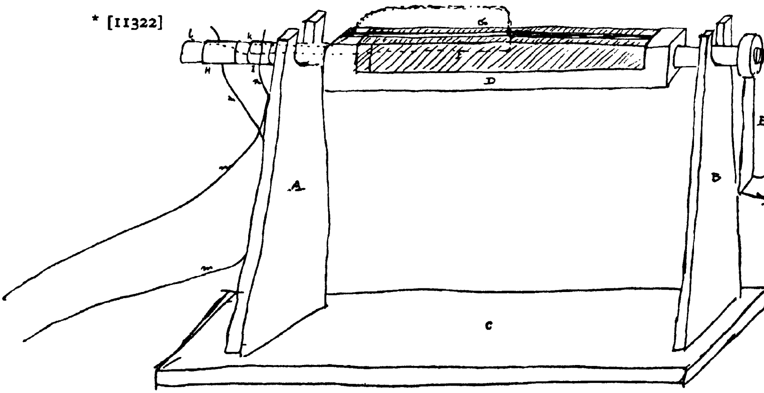
11320. Raised the box of the torsion balance a full inch so as to carry the beam farther up. Still when 20° from normal position and with torsion of 180° or even 360°, the magnet excited by 20 pr. of plates could draw it slowly in towards the mean or normal line.

11321. When the tubes Ox. and Nit. were now hung on, the action of the magnet on the oxygen was very evident–but it was slow, for the weight to move is very considerable.

11322*. An apparatus for expts. on the revolutions of wires about magnets in reference to the condition of the lines of force about a magnet, and the essential conditions requisite for the evolution of Electric currents. A, B are two wooden feet fixed on a base C—the feet support a framed axle D which can rotate by means of the handle E. The axle is of wood but is cut away so as to receive two bar magnets F and G, each 12 inches long, 1 inch broad and 0·4 of an inch thick. In cutting away the wood, the central part of the axle is left as a thin slip about $\frac{1}{10}$ or $\frac{1}{12}$ of an inch thick, so that the magnets when in place do not come together but are so much apart, and being tied in their places may then be revolved with the axle without disturbance. This separation is however cut away in the part towds. the pillar A, and so also is a corresponding groove in the axle over A, so that a wire may lie in the axis of the two magnets as far as the middle, as indicated by the dotted line and its direction.

11323. This wire is brought into communication with the Galvanometer as follows. H and I are two copper cylinder rings fixing tightly on the wooden axle D. When the covered wire is in its place, the two copper rings being away and the two ends of the wire at *k* and *l*, these are made bright for half an inch, then a little wooden block placed between wires *k* and *l* where I will come, and then I being pushed into its place, makes excellent contact with the end *k*, and all is wedged tight. Then H is put into its place and a little wedge pushed in so as to fix the end *l* against its inner surface. Thus the cylinders H and I form the terminals of the covered and insulated wire, whatever may be its

* [11322]



arrangement at the magnet. Two wires, *m*, *n*, were then fixed against the side of the Support A and one end of each cleaned and sprung so as to bear against the cylinders H and I, and their other ends were connected with my Galvanometer. That the circuit was complete and the connexions perfect was ascertained by a small thermoelectric circuit or by a piece of iron and copper wire touched by the tongue or hand.

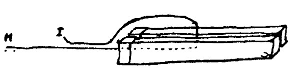
11324*. In the first arrangement the covered copper magnet wire was led from cylinder H along the axis of the two magnets (which had their like ends in the same direction so as to act as one magnet and produce one system of curves), and then rose from between them at the middle part and proceeded above, descending outside and from over the ends until it terminated at I. The object was to place one part of the wire in the axis of the system of curves and the other part so that, in turning the whole, it might be considered as cutting the system of lines external to the magnet. The connexion was perfect. But no motion of revolution of this system produced the slightest effect at the galvanometer. Though 30 revolutions could be made in the time of one swing of the needle, still no effect was produce[d].

11325. The direction of the revolution was made in the contrary direction, but with the same negative result (11584).

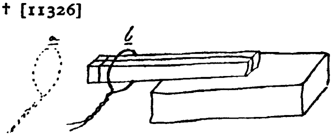
11326†. In order to ascertain that the absence of effect was not due to any want of power in the magnet or of delicacy in the Galvanometer, the magnets were taken off the frame and laid together on the table in the same relative position, and the wire from the galvanometer was formed into one loop so as to be able to pass over the end; in which case it would cut through nearly the same amount of lines of force as in one revolution of the former arrangement (11324). Now a single motion from *a* to *b* was able to deflect the galvanometer needle a degree or more, and the reverse motion back from *b* to *a* did as much in the contrary direction.

11327. The swing of the needle required many seconds, and it was easy in that time to carry the loop or ring from *a* to *b*, to break contact, return to *a*, to make contact and go again to *b*, so as to combine into one impulse on the needle several of the effect of carrying the ring from *a* to *b*, or from *b* to *a*, the

* [11324]



† [11326]



galvanometer connexion being at the same time complete. Now 5 or 6 such united efforts gave a deflection of the Galvanometer needle of several degrees, and it was easy thus to increase the deflection until it amounted to 10° or 15° on each side of Zero, or a difference of 30° or 40° on the whole. So there is plenty of power in the magnet and delicacy in the Galvanometer.

11328. Once on of the loop is equal in section of curves of force to one revolution of the Magnet and wire, supposing the lines of force are to be considered as having no motion with the magnet.

11329. Arranged a second wire as before (11324), entirely free from all suspicion of contact in the wrong place or want of contact in the right place. The circuit was good, but there was not the slightest effect at the Galvanometer by 20 revolutions of the magnet and wire—or with revolutions first one way and then the other, corresponding with the time of Vibration of the needle.

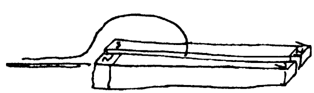
11330. When *the magnet* with its system of forces and *the wire* move with the *same angular velocity* in the *same time*, there is no generation of an electric current or tendency to such a current in the wire. This is a broad and fundamental fact (11584).

11331. I pulled the wire out from between the magnets and disposed it in various way[s], either all outside or partly outside, and in every possible manner consistent with its contact with the two cylinders H and I (11323), so as to give it all possible shapes and dispositions in relation to the curves of force about the marked or N end of the magnets—but the result was the same. When it moved with the same angular velocity as the magnet, no current of electricity was produced (11586).

11332*. I took one magnet out and turned it end for end, so that the wire was between and about an N and an S pole—but the result was absolutely the same. This was to be expected, and the experiment was made only to clear away objections and give an experimental answer.

11333. As the wire passes between two magnets in like position and then issues out from between them at the equatorial parts, I have assumed that it is as if the wire passed down the axis of a solid single magnet and then issued out through its side and went into and across the region of the external lines of force; and I have supposed that the part at the center would *not cut* or intersect

* [11332]



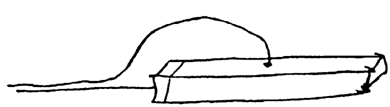
lines of force during the revolution, whilst the exterior part of the wire would, and I looked for a difference which would produce a current. But considering the wire in relation to either magnet, it is evident that both parts of the wire are *external* to the magnet, and that therefore as the magnet and wire moves round, even if there were a tendency to the formation of a current in the more distant part or external part of the wire, so there would also be a tendency in the other or nearer part of the wire, and as these parts of the wire move with equal angular velocity and cut so to say the same curves, so the currents tending to form would be equal, and being opposed in their course through the wire, would nullify each other. Hence *no current* would be the result. What would be true for one magnet would be also true for the other, and so neither of them separately could produce any current by such an arrangement as this.

11334. But when both were conjoined they produced no effect, though they then represented one magnet with the wire passing down the axis. So by parity of reasoning, if a single magnet be considered as made up of an infinity of linear magnets, like very thin wires, each of these infinitely thin magnets would be in the same relation to the wire as either of the above magnets (11333), and therefore the sum of their actions could produce no result of a current.

11335*. Hence if a wire did actually proceed down the axis of a solid magnet and coming out at the equatorial then proceeded outside as before, still when moving with and as the magnet, no current should be produced in it. Hence also, if the magnet itself were substituted for the wire, i.e. if cylinder H (11323) were connected by an axial wire with the end of the magnets by its wire *l* now cut short, and cylinder I by its wire *k* with the equatorial part of the magnet, such an arrangement ought not to give a current. The latter adjustment was made and the circuit, now partly through the matter of the magnet, tested and found perfect. But no amount of rotation of this system caused the least effect at the galvanometer when all turned together and with the same angular velocity.

11336. All this shews that, when the magnet and the wire move together, there is no induced electric current.

* [11335]



11337. When the wire moves independantly of the magnet, or the magnet independantly of the wire, then there are currents produced, as is well shewn by hundreds of the old experiments. Still, I should like to verify that by this form of apparatus, so as to have a true notion of the relation of cause to effect, and of the place where the current is excited. Tried one expt. in which the wire was moved in the same direction across the curves but without the magnet, and obtained a slight result, but want a better one.

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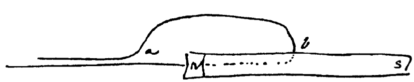
11338*. The Magnet was fixed in position and the wire loop so arranged that it could be moved; i.e. the part from *a* to *b* could be moved round a line from *a* to *b* as axis, the flexure of the wire at *b* being permitted by the metal itself. When it was moved in one direction, the contact of the galvanometer wire throughout was effected and when moved the reverse way interrupted. In this manner, the passage of *a b* thrgh. 180° could be observed as to its effect at the galvanometer—or 5 or 6 such passages in one direction could have their effects successively added together. When the wire was moved only once, the needle was affected. When five or six moves were added together, the needle was deflected 2° or more (11585).

11339. The motion of the wire in one direction made the needle go one way. When the wire moved in the other direction the needle moved the other way. By combining the effects so as to urge the needle in one direction for a time and then in the other direction so as to add momentum and current effect together, a good swing of the needle could be obtained amounting to 20° or more.

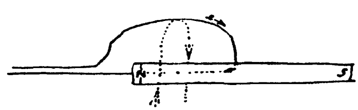
11340†. The direction of the current produced is of course in accordance with the laws formerly determined, and is as follows; i.e. when the wire moves thus through the lines of magnetic force, the electric current is as marked by the small arrow *e*→.

11341. So when the *magnet is still*, and the *wire moving* through and across the lines of magnetic force, an electric current is generated. Is it generated in the moving wire or in the wire within the magnet? The current cannot be generated here in the body of the

* [11338]



† [11340]



magnet, for that is in the present case insulated from the wire and the Galvanometer.

11342. Now the wire (11338) was held so that it should not move when the magnet was made to pass through a half revolution, or if it moved, that it should pass only in a vertical plane, so as not to intersect any lines of force that might not themselves be considered as changing their places, the object being now to move the magnet on its axis through 180° and not the wire. The impulses generated by successive moves could be added together as before (11327, 11339, 11585).

11343. The Galvanometer was now well affected by the moving magnet, and the deviation of the needle was in this or that direction as the magnet was moved either way. But the direction of the current was the reverse of that in the former case; i.e. when the magnet was moved 180° in the same direction as the wire was moved before (11340), the current at the Galvanometer was in the contrary direction. This should be so as regards the mutual relation of the Magnet and the wire—for to move the wire over the magnet in one direction is relatively the same thing as moving the magnet under the wire in the contrary direction, and therefore to move the wire over the magnet or the magnet under the wire in the same direction ought to produce contrary currents.

11344. In the former case (   ), when the wire was moving through the curves in the *direction indicated* (11340), the current induced in it was from the parts near the N. end to the parts near the equator of the magnets. In the latter case (11343), when the magnet was moving in the *same direction*, the current in its mass is also from the N. polar to the equatorial parts. So that the matter of the wire and of the magnet are alike in their affection in relation to the power of the magnet, i.e. to the system of magnetic force.

11345. When the magnet is still and the wire is moving, it seems unlikely that the current should be generated any where else than in the moving wire; for its motion or quiescence makes all the difference. But then, when the magnet is moving, where is the current then generated? In the wire across which the curves, that may be supposed to move with the magnet, are passing? Or in the magnet, which may be supposed to be moving (as the wire

did) whilst the curves are considered as still? Do the lines of force revolve with the magnet or do they not?

11346. The last experiment is the same (11343) as my old one of spinning a magnet and obtaining the current by holding wires from the galvanometer against the axial and equatorial parts. I should say that in the last experiment the wire was not continuous and insulated between the two magnets, but touched at the N. polar and at the equatorial parts.

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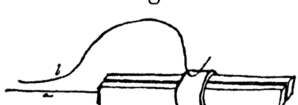
11347*. In order to obtain a better contact for the wires, etc. when the wire or the magnet has to move, the one without the other, I have fixed a copper ring round the equatorial parts of the two magnets and in contact with the magnets, and so by making the wire ends press against this well cleaned copper cylinder, I could obtain contact when required and yet freedom of motion for the magnets to revolve.

11348. In the first experiment, *a*, covered wire, proceeded in axially at the N. end of the compound magnet and was connected metallically at the central parts corresponding to the plane of the equator. The end of the second wire, *b*, which was a fixed wire, pressed on the equatorial copper ring. *a* revolved with the magnet but *b* did not; the ends of course went away to the galvanometer. On revolving the magnet, there *was a current* at the galvanometer. The current was in either direction according as the magnet was revolved in the one or other direction, and was the same in direction as before. Here the part of the Magnet in the electric circuit is the central part only, and not any serious portion of its length. It is just that part which we may consider as most apart from the influence of the *external lines* of force. If these lines are to be considered as continuous (in closed circles) within the magnet, then this part will include them all.

11349. Ten revolutions of the magnets gave a deviation in either direction of nearly 5° at the galvanometer.

11350. At first there seemed to be some effects produced dependant upon the *rate* at which the revolutions of the magnet were made, or upon the distance at which the fixed immoveable part of the wire was from the magnet: and as these effects seemed

* [11347]



to interfere with the definite character of the force, they were worked out first; for neither difference in velocity nor distance of the fixed wire from the magnet ought to affect the amount of electricity thrown into the form of a current. That should be affected only by the amount of lines of magnetic force passing over the wire or passed through by the wire.

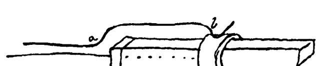
11351*. At present the outer part of the wire, being a fixture, is about an inch distant from the magnet in the part from *a* to *b*. The contacts were all good and continuous, as they could be by the present arrangement. Ten revolutions of the magnet on its axis in moderate time gave a deflection of the galvanometer needle of 9°. Ten turns in slower time gave a declination of $7\frac{1}{2}°$ or 8°. Ten turns in moderate time gave 9°. Five turns in slow time gave 4°. Five turns in moderate time gave $4\frac{1}{2}°$. These results occurred again and again, and continued when the bearings at the rubbing surfaces were increased or steadied. The velocity evidently made a difference.

11352. Again. Ten slow revolutions gave 8° of deflection. Ten slower revolutions gave about the same. Ten very slow revolutions gave only 5° of declination. Ten still slower gave only 3° of declination. Ten quicker revolutions gave 7°. Ten still quicker gave 9°·5. In the latter case the momentum of the needle carried it on a degree or more (included in the 9°·5) after the rotation was concluded: but this was not the case with the slow motion of rotation: then the needle returned the instant the rotation ceased and had been evidently held in its place against the earth's force by the current due to the rotation. Ten very quick rotations were over before the needle had swung through 6°; but the rotation having ceased, it went on swinging to nearly 10°.

11353. Increasing the number of rotations makes this effect more distinct. Twenty moderate rotations gave a deflection of 8°, and the 8° having been attained some time before the rotations were over, the needle returned the instant the magnet rotation ceased. But twenty quick rotations gave 15° before the rotation over and this increased up to 17° after the rotation had stopped by the momentum of the needle.

11354. So the cause of the difference of effect by quick or slow rotation of the magnet is clear (11594). It is not due to any

* [11351]



difference in the *whole amount* of Electricity evolved by a given number of revolutions of the magnet, for that appears to be the same whatever the velocity with which the revolutions are made: but a slow velocity gives a feeble current for a long time and a quick velocity gives a strong current for a short time. The feeble current can only deflect the needle a certain number of degrees, and if it be continued after that deflection is attained, it is employed only in sustaining that deflection against the earth's force tending to draw the needle back again, and has no power to increase it. But a stronger current can produce and sustain a much larger deflection, and consequently, if the time of revolution in both cases (being shortest in the quick revolutions) is enough for the needle to have attained its maximum deviation, that declination will be greatest for the quick velocity. Or, if the time in the slow rotation is enough to give its maximum declination, then the much shorter time for an equal number of quick rotations will give a larger declination. It is only when the time for the slower rotations is so small that the corresponding declination is not at its maximum, and the needles therefore continue to swing on afterwards, that an equal number of quicker vibrations gives the same amount or nearly so of declination (11529).

11355. The difference of effect with quick and slow rotations is in fact a result of the opposition of the current in the galvanometer wire and the earth's force on the Magnetic needle. When the current cannot carry it to or hold it at a great angle of deflection, then only a smaller angle is attained to. The greater angle of declination belongs to the quicker rotation and more intense current.

11356. The next point was as to the *effect of distance* of the still part of the wire under the moving magnet (11350), or of the moving wire from the still magnet. Of course a change in distance ought to make no difference. Otherwise the results before described, with the loop of wire at different distances and in different positions (11331), could not be true. But to confirm the principle some direct experiments were made.

11357. The still wire being near the magnet (11351), about 1 inch off only, ten revolutions of the magnet with a moderate velocity gave a deflection of 9°. Then the wire being farther of[f] and

about three inches distant, ten revolutions gave 9° of deflection as before. The wire was then placed still farther off, perhaps 5 or 6 inches, but still the ten revolutions gave the same 9° of deflection as before.

11358. Hence the distance of the wire from the revolving magnet makes no difference in the result, the same amount of Electricity being evolved by the same *angular journey* in the one case as in the other.

11359. We have seen already that if the wire and the magnet move together, whatever the distance apart (11324, 11331) there is no current produced; and we know that if the magnet were still and the wire moving, the effects are the same as if the wire were still and the magnet moving in the reverse direction (11343). Hence if the wire move concentric with itself and always with a uniform velocity, the current produced in it is inversely as the distance and simply as the distance.

11360. In order to make this law harmonize with that of the square of the distance, we must consider that the motion is only in one direction. In the other direction, increase or diminution of the wire makes the element, and supposing the pole a center, this increase would be as the distance. This element probably affects the *intensity* of the current produced in the wire and not its quantity. It may be important in relation to natural circumstances, i.e. in relation to electricity evolved by the revolving globe.

11361. The wire being at the greatest distance as described (11357), the effect of quick and slow rotation was ascertained as before (11354) and exactly the same results obtained. Thus ten revolutions very slow gave a deflection of 3°. Ten quicker turns gave 8°—ten very quick turns gave 8°·5—the same—the same; the swing was not over here when the ten turns were completed and so the maximum effect was reached. Twenty turns slow gave only 6°—twenty turns quick gave 15°—the same result again: all very consistent.

11362. Now the wire was put near again—then twenty quick turns gave the same amount of 15°.

11363. I found that, continuing the rotation of the magnet, when a very slow motion would hold a deflexion of only 2°, a very

quick motion, still only such a one as I could give by a simple handle to the present apparatus, would hold it 28° from Zero.

11364. So there is no difference between a near or a distant position of the wire external to the magnet—all other things being the same.

11365*. All this time the axial wire has proceeded up to the central part of the two magnets, being insulated from them except at the place of contact there (11348). Now made it end at the N pole of the magnet, so that half the length of the latter was in the circuit. The result was in all respects exactly the same as before; i.e. 9° of deflection for ten turns at moderate speed. Ten slow revolutions gave 4°. Ten quick turns gave 8°·5, the needle swinging after the revolution was over. Twenty slow revolutions gave 3°, twenty moderate revolutions gave 7°. Twenty quick revolutions gave 16°.

11366. The magnet evidently replaced the central wire and did neither more nor less. Whether the current is in part or altogether evolved in this central wire or its equivalent—or in the disc in the middle of the magnet reaching from the central wire to the copper ring—or in the outer wire—remains to be proved (11588).

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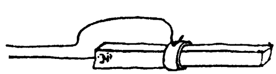
11367†. The apparatus was now adjusted so that the central or axial wire could rotate either with or without the magnet. The connexions were perfect and all in order. Then with the *central wire rotating* but the magnet and external wire quiescent, there was *no current* produced—not by any amount of rotation (11590). The wire was covered so as to be insulated from the magnets, except at *c* in the center, where it made contact with them.

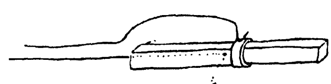
11368. Then the reverse action was established, i.e. the magnet and outer wire were rotated and the axial wire retained quiescent: but still no current was obtained. This was as it should be (11591).

11369. Revolving the inner or axial wire with both the magnet and the outer wire produces no effect. This has been shewn before (11324)—and when it was revolving with the magnet, the outer wire being still, it in no way changed the action or its amount (11349), so that whether it be quiescent or revolving in either

* [11365]



† [11367]

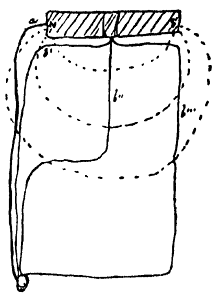


direction makes *no difference* in the result. Such motion has no effect.

11370. The inner wire therefore is, under the circumstances, a mere conductor and not a generator of the current. So when the magnet is in the place of the inner wire (the latter making contact at the pole instead of the middle (11365)), it then, so far as it replaces the inner wire, is a mere conductor.

11371. When the inner and outer wires were still and the magnet revolved, then the proper currents were obtained. Also when the inner and outer wires moved together and the magnet was still, then there was a current in the reverse direction to the former one, as it should be (11343, 11592).

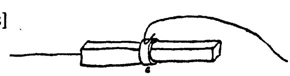
11372*. The outer wire was now led off over the S pole of the magnet. Still the revolution of the magnet gave exactly the same result as before. This does not prove that the current is not generated in the wire by the moving system of forces of the moving magnet, because whether the wire, being outside, went away over the N or S pole, the revolution of the magnet would equally determine the same direction of current to or from the part touching the equatorial copper ring.



11373. If the wire were led perpendicularly away from the copper ring *c*, still the same result occurred. But in fact we can establish no difference in the direction in which the wire is led off to the galvanometer: for it is manifest that if *a* being the axial wire, b', b'' and b''' be the external wire in different positions, then whatever magnetic curves cut the wire in one position will cut it also in any other position. The distance of the wire at the supposed place of intersection has been shewn to be unimportant (11356).

11374. If the axial wire were led out at the S pole, the effect would still be the same for the same revolution of the magnet. This is manifestly the simple converse of the experiment just described (11373). In fact the direction of the current may be considered as determined by that equatorial disc of the magnet which is between the end of the axial wire at the center and the ring of copper outside.

* [11372]

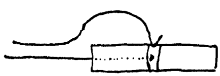


11375. I have procured different kinds of wire both as to thickness and substance. They are as follow:

0·04000	No. 1. The covered wire – copper metal	$\frac{1}{25}$	of an inch in diameter
0·01351	2. Uncovered copper–thin . . .	$\frac{1}{74}$	of an inch in diameter
0·06666	3. Do. . . . . . . thick . .	$\frac{1}{15}$	Do. . . . .
0·05000	4. Uncovered lead wires. . . .	$\frac{1}{24}$	Do.. . . . . . .

	The diameters therefore were of	and the sections were as
No. 1.	2·96	8·76
2.	1	1
3.	4·93	24·3
4.	3·7	13·7

11376. As to the length of wire in the connexions, there is about 25 feet of No. 1 wire in the running connexions. That in the coil of the Galvanometer I do not know.

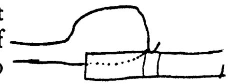


11377. Now experimented as to the different positions of the axial wire so as to limit in some degree the place of generation of the electric current. The axial wire, insulated in its course, made contact at the center of the magnets (as before (11365)). The external wire was fixed–and the magnets rotated so that the central disc or portion of the magnets was that included in the circuit and that which rotated. Ten revolutions of the magnet was the quantity of rotation given, because that could be effected before the needle came to a stand–and there was therefore time to turn and see the extreme deflection–and that was known to be a definite quantity (11344).

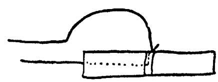
11378. Ten right revolutions gave a deflection of 7°·5. Ten right gave 8°. Put on a little more pressure at the slipping equatorial contact. Then ten right revolutions = 7°·5. Ten left revolutions gave 8°·5.

11379. The needle appears as if it could vibrate a little farther on one side than the other, probably from some difference in the set of the helix–or the direction of the friction at the moving contacts–or in the influence of external objects, as window weight, etc.–themselves influenced differently at different times of the day; but it is easy to correct the effect of these variations by adjusting the scale to the needle continually and by taking in all cases the results both of right hand and left hand rotation.

11380. Now the axial wire was led up to the edge of the equatorial copper ring, being insulated all the way from the magnet, so that no part of the magnet was in the circuit and the moving part of the wire was the curved part between the magnet equivalent to a radius. It is known that the axial part of the magnet by rotation does nothing ( ). Ten right revolutions = 8°. Ten quicker gave 8° of deflection. Ten moderate left revolutions gave 9°. Ten quicker left gave 8°. Ten moderate left gave 8°·5. Ten moderate right gave 8°. The general course of the current is the same as before (11340). The amount excited in the two experiments (11378) is the same.



11381. The axial wire was now led along the axis to the center and then turned up at right angles and made contact with the copper equatorial ring, being insulated elsewhere. The magnet was not in the circuit and the case was like the last, except that the wire was axial to the center and then at once radial. This radial part was the moving part and of course is only 0·5 or 0·6 of an inch in length.



11382. Ten right revolutions = 7°·8 – 10 left revolutions = 8°. Ten right = 8°. Ten left = 7°·8. Ten left = 7°·8. Ten left = 7°·8. Ten right = 9°. Ten right = 8°·4 – 10 left = 8°·4. Ten right equalled 8°·2 of declination.

11383. So the current evolved is the same amount as in the other cases (11378, 11380). It is definite for all three and alike when the circuit is a sufficiently good conductor. Being at the center or at the edge of the equatorial ring makes no difference.

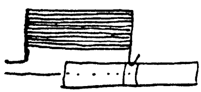
11384. Proceeded to experiment upon the effect of variations of the external wire, making first of all the contact of the axial wire at the center, and always causing the magnet to revolve and the external wire to be fixed. If it be supposed for a moment that the solenoid of power revolves with the magnet and is cut by the external fixed wire, i.e. that the external wire is the place of generation of the current, then variations of this wire might be expected to produce some effect. It is almost certain that whether the magnet revolve within the outer wire, or the outer wire revolve round the magnet, the results are in quantity the same.

11385. The wire No. 1 (11375) has already given a certain constant result of power. Now made the external wire No. 2 or

fine copper; it has little more than a ninth part of the mass of the former wire. Ten right revolutions gave 7° of deflection. Ten right = 7°·5. Ten left = 8°·4. Ten left gave 8°·4. Ten right gave 8°. Ten left gave 8°. So this thin wire is the same as the thicker one in its power of generating or affecting the induced current—the sum of power induced is the same.

11386. Made the external wire No. 3 (11375) or thick copper, being about 24 times the mass of the last. Ten right revolutions gave 7°·5. Ten right = 7°·5. Ten left = 8°·5. Ten left gave 8°·4 of deflection. The results are the same as before, so that copper wires, being in mass as 1 : 8·76 and 24·3, evolve precisely the same amount of power.

11387. As regards mere conductibility of a current generated elsewhere, this should be so—but as respects the supposed generation in the wire of circuit, then it might have a different bearing.



11388. I made 11 of the thick wire No. 3 into a band, well connected at both ends, so that each of the 11 could be subjected to the curves of force and throw their joint effect forward by the conducting wires to the galvanometer: the amount of copper here submitted to the force is 267 times that in the thin copper wire (11375). The 11 wires were in a plane standing as tangent to the rotation of the magnet. Ten right revolutions gave 8° of deflection. Ten right gave 9°. Ten left gave 7°—adjusted, then Ten left gave 8°·5. Ten right gave 8°. Ten left = 8°. Ten right, 8°·2.

11389. Being left for several hours, then Ten left rotations = 8°·5. Ten left, 8°·5. Ten right, 8°. Ten right, 8°.

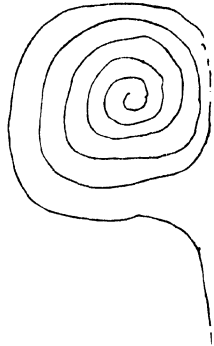
11390. Now placed the 11 wires in a plane passing through the axis of rotation and therefore perpendicular to the former. Ten right revolutions = 8°. Ten right = 7°·5. Ten left = 8°·4. Ten left = 8°·3. Ten right = 7°·9.

11391. So whether it was in one plane or the other, the results were the same and the same as the thinnest copper wire.

11392. The external wire was now of *Lead* or No. 4 (11375). Ten right revolutions produced a deflection of 7°. Ten left of 8°. Ten right of 8°·2. Ten left of 8°. Ten right of 7°·9. Ten left of 8°·5. So lead produced no difference from copper.

11393. To ascertain whether the current induced by this magnet and its present arrangement (11322) could pass through bodies not metallic: the outer wire was made of two pieces of the thick

copper No. 3 (11375). Two feet of each, being well cleaned, was made into a spiral, the spirals placed in a tumbler of sea water and the other ends connected properly at the equatorial part of the magnets and at the Galvanometer wire. There was much disturbance of the needle at the putting in of the spirals to the sea water, but the action was instantly over and the needle came to rest. Then depressing one only a very little so as to immerse, fresh water caused great disturbance, but this instantly ceased when all was at rest.



11394. Being still and in order, the magnet was rotated, but not the slightest signs of Electricity appeared at the Galvanometer by any alteration or arrangement of the rotation.

11395. Hence the current, wherever generated, seems too feeble to break up the interposed electrolyte, whether that be considered as the water or the salt or the films of chloride of copper on the wires. This is rather against the idea of evolution of static electricity between the poles (   ) but still not at all conclusive.

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11396. From all that has appeared, there is every reason to believe that it is absolutely a matter of indifference to the final effect, whether the magnet rotate within the external wire or the wire rotate in the contrary direction without the external magnet; the axis of rotation and the amount of rotation remaining in both cases the same. Hence it might be assumed that if the wires in their variety (11384, 11392) already experimented with, if they had been rotated instead of the magnet, would give precisely the same result[1]. But if the generation of the current be assumed as existing in the *moving part* then that part has been unchanged during the experiments and it was just possible that such differences as shewed no difference as conduction seeing that the smallest wire was abundantly sufficient for the greatest current produced might shew differences if when moving they became the generators for then the amount of curves intercepted by their moving mass was very different under the different circumstances.

11397. I could not so arrange as to make the amount of revolu-

[1] The meaning of the following passage is in doubt, and it has been left unpunctuated, as in the original.

tion of the external wire equal to the revolution of the magnet in a sufficiently short period of time and could not therefore compare one with the other. But I could arrange so as to vary the external wire, and in all cases to accumulate in succession Six half rotations (180°) in one direction, the accumulated effect on the needle, and as the left handed half rotations were most convenient for working, these were always employed.

11398. The covered copper wire No. 1 (11375) was first employed externally and placed in good rotating contact at the two ends; the magnet was fixed and the axial wire went to the center of the magnets. Six half rotations gave 2° of deflection. Eight = 2° full. Six = 2°. Six = 2° nearly. Eight = 2°·2.

11399. One thick copper wire No. 3 (11375) made the external wire. Six half rotations = 2°. Six = 2°. Six = 2° nearly. Eight = 2°·1. This wire is, so far, as the thinner wire.

11400. The very thin copper wire No. 2 (11375) made external moving wire. Six = 1°·5. Six gave 2°. Six gave 2° nearly. Six gave 2° nearly. Eight gave 2° full. Six is the best quantity for comparison. Six gave 2° nearly. Eight gave 2° full. The effect of this very nearly as the others. From the flexibility of the wire it was not so easy to make every part pass each time through the 180° of rotation.

11401. Now the Eleven thick wires (11388) were employed as the moving external wire and were placed in the plane passing through the axis of rotation and also of the magnet. Six half rotations gave 2° nearly of deflection. Again same result. Again the same result.

11402. The eleven wires were now placed in a plane at right angles to the first and as tangent to the direction of rotation (11388). Six gave 2°. Six gave again 2°. Six gave again 2°.

11403. So that all these varieties of the moving wire gave one constant result, exactly the same in all the cases. When the wire was made *lead*, still the same result was obtained. I therefore have no doubt that if Ten revolutions of the wire outside of the magnet could have been effected, I should have obtained exactly the same amount of deflection as when the magnet made ten revolutions the contrary way (11396).

11404. Suppose ten revolving outside wires, each with its own fixed circuit wire and a galvanometer to each. Each wire in Ten

revolutions would affect the Galvanometer 8° (11385, etc.). But if the ten revolving wires were all laid on to one galvanometer, the sum of the action of the whole would be only 8° (11388 etc.) under such an arrangement as that just employed. Or suppose a copper plate revolving concentric with and over the axis of a cylinder magnet*: it will in its revolutions cut the magnetic curves and tend to have currents produced in it radially, i.e. from or to the center. Then with one wire to the center and another leading from *a* to a galvanometer, a current of a certain amount will be produced and ten turns or revolutions of the plate will give a certain deflection, say 8°. Then other wires from the center and from *b* going to another galvanometer will also give a deflection of 8°—and so on for other wires originating at the center and any where in the circle *a*, *b*. But if two or more of these sets of wires are connected with the same galvanometer, then apparently its deflection will not be increased (11388). Must try this, and if it be so, ascertain the cause (p. 2322[1]).

11405. In that case, if all the parts from *a* by *x* to *b* of that circle was permanently connected with the center by short *immoveable* wires, still a wire applied any where between *a* and *b* and going to the Galvanometer, and thence back to the center, will still carry off its full amount of 8° to the Galvanometer. If not, then what is the passage of the two cases one into the other?

11406. As quickness or slowness of revolution seems to make no difference with perfect conductors (11354), so with a given wire the electricity at the Galvanometer is the same for the same angular journey—and therefore the quantity is proportionate directly to the lines of force passed through.

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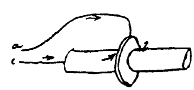
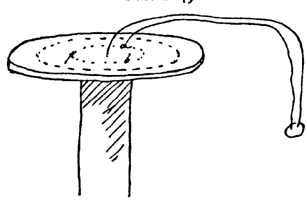
11407. The force of a given magnet is definite and may be considered as represented by its curves. The Northness and Southness are exactly equal, otherwise the curves could not be definite.

11408. If we consider a revolving magnet with its external wire (11324, 11335) and imagine its equatorial part to be extended outside, then so much of the disc as is outside the magnet must, in moving with it, tend to have currents set up in it in the contrary



[1] Par. 11490.

* [11404]



direction to those in the parts within the magnet, for the part from *a* to *b* has contrary currents from those from *c* to *b*; i.e. rotating both together and therefore in the same direction, the tendencys would be as shewn by the arrows and equal ( ).
11409. And as shewn before, the equatorial disc part of the magnet is equal to all the wire outside (11335).
11410. The curves therefore exist within the magnet as well as without; but within they are in the contrary or return direction.
11411. A section across the middle of the magnet is equal to a whole section outside, i.e. from the equator by or over one pole to the other equator. A section by a radius is equal to that by an external wire from the equator to the pole (11348, 11377).
11412. Whatever the condition of the interior of the magnet: it has by this test of inductive action on a wire, the same kind and amount of power as the outside, and so is in full analogy and similitude with an electro helix.
11413. The intensity of the curves of a magnet vary greatly at different distances from a magnet: and we want a standard of intensity or power as to these lines of force, that we may have from it a value for other intensities greater or less.
11414. But the amount of force is definite and the same for every section of all the curves, wherever it may pass: as is shewn by the external wire from the pole to the equator (11364), for it gives the same induced result of electricity, whatever distance it be placed at or whatever direction it may take.
11415. The amount is the same also for any two sections of the same curves, whatever difference there may be in the intensity of those curves at the two sections. This is shewn when the external wire is made into a loop (11331) of any form: for then there is no current, the sections having equal power though greatly varying in intensity.
11416. Hence it follows that whether the curves are intersected directly or obliquely makes no difference provided they are intersected. The effect depends upon the number of curves intersected. A wire moving obliquely may intersect fewer curves and therefore have a feebler current evolved in it; but if it intersected only the same curves directly across, it would have no larger a current.
11417. So with a given moving wire (11397) or with a given wire under which a magnet is moving (11347), the quantity of

electricity generated is directly as the amount of curves passed over or through.

11418. With the same curves therefore it varies directly with the velocity of the motion.

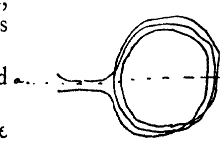
11419. It varies directly with the (angular) Journey.

11420. Or with the time; velocity remaining the same.

11421. A magnet of a given strength therefore might be made to supply a standard of Electricity for feeble currents.

11422. Whether the intensity would vary with the length of the wire is a question at present; i.e. would twice the length with half the Journey give the same quantity and intensity? Or half the quantity and twice the intensity? Or the same quantity and twice the intensity, or what result?

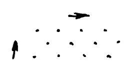
11423. A moving wire is a very valuable examiner of magnetic curves—and a new one, i.e. quite distinct in its character from ordinary magnetic action, yet apparently exceedingly definite, clear and instructive. Perhaps we may obtain some new characters by it.



11424. By such a form of it as this, revolving on the axis *ab* and combined with a commutator, we might examine many things.

11425. For instance, one might examine a line of magnetic force on all sides: for I am not as yet quite sure that differences may not be impressed upon it.

11426. Again—is the amount of necessity the same in two directions, one parallel to the plane of the Magnetic meridian, the other in a plane perpendicular to the former: always meaning in a field of force with parallel lines, as for instance the earth's magnetic force? There should be no difference.



11427. Can there be concentration in one direction and not in the other?

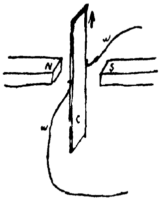
11428. What will a ring of 2 inches diameter (11424) do in evolving electric force by the earth's lines—with my Galvanometer?

11429. Or one of a foot or of two feet?

11430. Or of one wire—thick and thin—and a cylinder?

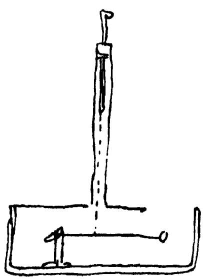
11431. Or rings containing 1, 10, 20 . . 100 convolutions? Then the proportions of these to each other?

11432. Their effects on a galvanometer with a single wire—thick; or three or four thick convolutions—and other such variations?



11433. Experimented with the endeavour to develope *static electrical phenomena* from magnetic forces–reasoning upon the general nature of the lines of magnetic force. Let N, S be the two poles of two magnets or of a horseshoe magnet; lines of force will exist between them in a horizontal direction. Let *c* be a plate of copper placed equatorially or across these lines and able to move up or down–and let *w*, *w* be two stationary wires touching the edges of the moving copper and leading away to a galvanometer. As the copper plate is moved *up*, a current will go across it in *one* direction through the conducting wires away to the galvanometer, and as it is moved *down*, a similar current will go in the other direction.

11434. The point is, suppose *c* a non conductor or *w*, *w* non conductors: would such motion, though it could not cause a current, tend to excite Pos. and Neg. Electricity correspondant to a current, and would the edges of *c* and the touching points of *w* leave each other in different electrical states through the tendency of the lines of force to produce a current? For though the current produced in conductors is of very feeble intensity, still it is by comparison very abundant in quantity, affecting the Galvanometer as a machine could not do unless very powerful, and if this current were stopped, the intensity might rise. Experiment only could determine this.



11435. *Electrometer*: a small disc of silvered paper was attached to one end of a fine well drawn thin stem of shell lac, and then this stem suspended horizontally by a single fibre of cocoon silk from a shifting rod inserted in a vertical tube. The tube was fixed on a box which enclosed the horizontal stem and preserved it considerably from air motions. Two stops were adjusted at the hinder part of the stem to limit the oscillations of the needle part. So when the silvered paper was charged, it was well insulated by the shell lac and kept its charge well; and it also was very free to move by attraction and repulsion. It was charged by exciting a piece of shell lac, then charging a small carrier by the lac and finally touching the suspended silvered paper disc with the carrier.

It was a good electrometer. The disc of silvered paper was about $\frac{1}{3}$ of an inch in diameter and the shell lac arm supporting and insulating it about 3 inches long from the silk suspender.

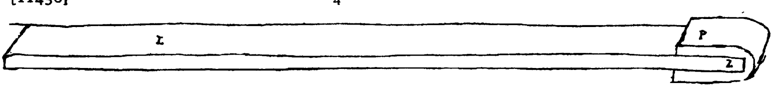
11436. The rubbers which were to be stationary in the magnetic field may be called S, S (Stationary); they were of various substances and thus constructed*. LL is a piece of wooden lath nearly the full size[1] and P is a piece of Gutta percha, which being made warm, has been applied to the end so as to present a regular rounded termination fit for a rubber. Other similar pieces of lath were finished of[f] at the rubbing end with shell lac, sulphur, etc. Others with flannel, white silk, tin foil, which was attached on to terminations previously made of Gutta percha. One was covered first with four thicknesses of flannel and then thin copper foil, drawn tight over that and nailed down.

11437. The part to move may be called R (running), and was a strip of flannel or a piece of white silk—or tin foil—or even a flat bar of shell lac or Gutta percha. It was to represent the moving metal plate (11433).

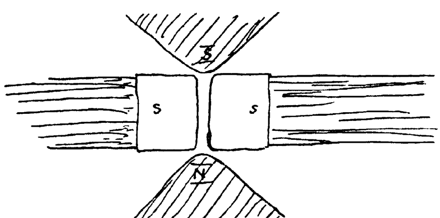
11438†. The following was the plan of arrangement at the poles of the great magnet, excited, when needful, by 20 pr. Grove's plates. $\underline{\mathrm{S}}$ and $\underline{\mathrm{N}}$ are the poles of the magnet and the initials correspond to the S. and N. ends of a magnetic needle. S, S are two rubbers fixed in their place by blocks and weights placed upon their further extremities to keep them steady. So the lines of force are powerful in the space between them. When experimenting, the moving body or R is to be placed between these ends, which then are to be held so as to press it—and when the magnetic power is on, the R is to be drawn up, so as to run between and rub against S, S. This may be repeated 5 or 6 times, always taking care that the motion of R is in the same direction when rubbing.

* [11436]

[1] Reduced to $\frac{3}{4}$ scale.



† [11438]



11439. S, S, flannel faces on Gutta Percha ends—R a slip of flannel. So all the rubbing surfaces flannel in its ordinary cold state and no magnetism on—there was no excitement of Electricity. The flannel was too damp. When warmed and dried, then it became excited irregularly; but when excited, preserved its charge well enough to be thoroughly examined.

11440. In these experiments, such bodies as shell lac and sulphur become charged and keep their charge tenaciously, but they may be discharged (   ) by breathing well on them and then wiping the surface with the same part (over a finger) of a silk handkerchief or a linen cloth in its ordinary state. S, S as well as R always has to be examined before an experiment, that they may be known to be not excited. The ends of S, S, when unexcited, will attract the charged disc of the electroscope, but that is because of the conducting wood within the shell lac or other body. The shell lac or sulphur only makes the action of the wood more manifest, because of their high specific inductive capacity.

11441. The flannel strip R may be discharged by laying it between two tin foils and pressing it for a time.

11442. Now the rubbing surfaces S, S, were made *shell lac* (11436) and the runner between, R. Whilst the flannel was unaired, it preserved no charge, conducting too well. But when the flannel was well warmed and aired, though it could be discharged and not excited by drawing it through *linen*, still when drawn through the *shell lac S, S*, it was well excited, becoming positive. Hence it insulates sufficiently to acquire a charged state.

11443. Then the friction was repeated, the Magnet power being on, and again the flannel R became altogether positive and the two shell lacs both Negative without respect to the direction of the magnetic lines of force. In repeated experiments with S, S Shell lac and the runner R flannel, no effects were obtained which shewed any relation to the direction of the Magnetic force. It seemed to be indifferent to these excitations.

11444. The rubbers S, S, being *shell lac* (11436, 11442), the runner R was also a flat plate of shell lac about 0·75 of an inch wide and 0·1 of an inch thick. Being at first discharged, the friction made R Negative and both S, S Positive. This effect was often obtained, but with some variations. Then, employing the

:tic power also, still the same or similar effects came out—
was no appearance of any action exerted by the magnetic
-all being *Shell lac*.

The rubbers S, S being *shell lac* (11436), the runner R
ıade tin foil, being a band of that substance of four thick-
. The rubber could not here be found charged, being un-
:ed, but the two rubber surface[s] might have given different
But whether without or with the magnet, the shell lacs
/ere both excited always and allways *both* Positive. So there
sign of any magnetic effect of excitation when one body is
ac and the other a conductor as tin foil (11451).

Sulphur was now the rubbers S, S (11436), and flannel
nner R. Before it was air[ed] it did not itself appear to
any charge—but the Sulphurs became both negative. When
aired and dried, then it became positive, making the sulphur
ve. The magnetic force made no difference.

S, S being sulphur, the runner R was made tin foil (11445).
·hether magnet or no magnet, both the sulphurs were left
ive. So here no peculiar signs.

S, S being sulphur, the runner R was made shell lac
). *Both* the sulphurs were rendered negative and the shell
sitive, whether with or without the magnet.

Flannel on Gutta Percha was S, S (11436) and the runner
flannel. There was the same uncertain result, whether in
of the magnetic field.

White silk on Gutta Percha was S, S (11436) and the
: R also the same white silk. The runner R became Negative,
er with or without the Magnet. Then made the Runner R
lks (in thickness), but whether with or without the magnet,
hese halves of the R became alike negative. They did not
e different states.

Tin foil on Gutta percha for S, S (11436) and *shell lac*
: runner R. The latter or shell lac became positive on both
and that whether with or without the magnetic action. The
appeared to make no difference. So the result the same
as when Shell lac was rubbers and tin foil runners
).

Copper on flannel for S, S (11436) and two white silks

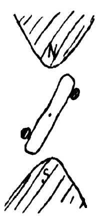
for runner R ( ). Both the silks of the runner became positive, and that whether magnetic power was there or not.

11453. With *Copper* for S, S, made the runner R shell lac (11444). Whether with or without the magnet–the shell lac was well positive, on both sides.

11454. With copper for S, S and the two white silk for R (11450), placed the rubbers with the line joining them equatorial, as all along (11438), and then turned the system 90° in a horizontal plane, so that the line joining them should be parallel to the magnetic lines of force and the plane of the rubber axial. There appeared at first to be a difference for the two position[s], the silk being frequently positive for the one and negative for the other, but on numerous repetition[s] and also with a single silk R, I could not find that there was any difference dependant on the magnetic state.

11455. The silk came out sometimes positive and sometimes negative, but I found that just after warming the silk, it came out of friction with copper rubbers Negative for both positions, and that if I then allowed it to cool awhile or wiped it with a slightly damp linen cloth, it was positive when rubbed against copper in either position–or with magnet or no magnet.

11456. *Tin foil* for S, S, and silk, white, with either axial or equatorial positions as above (11454); the silk always positive whether magnet or no magnet.



11457. Plücker says that if a piece of bismuth be placed between the magnetic poles, but so blocked that it cannot go far in the equatorial direction, then that when the current is taken off from the magnet, there is no particular effect produced; but that if the magnet be quickly reversed, there is an effect, the bismuth tending at the moment to be drawn axially, as if it retained opposite polarity for a short moment by its so called coercibility or incoercibility, i.e. its power of retaining a polar state for an instant of time. I tried the effect but could not perceive it. There is certainly a shake at the first reversion, but I think it results from the difficulty of blocking both arms exactly alike, and then the center of Gravity is not truly below the point of suspension, and so motion is produced at the taking of the repelling power. That

is done far more quickly by reversing the magnet than by merely taking off the battery (when the magnet falls slowly), and so I think this shake is produced. Perhaps induced currents may have a little to do with it.

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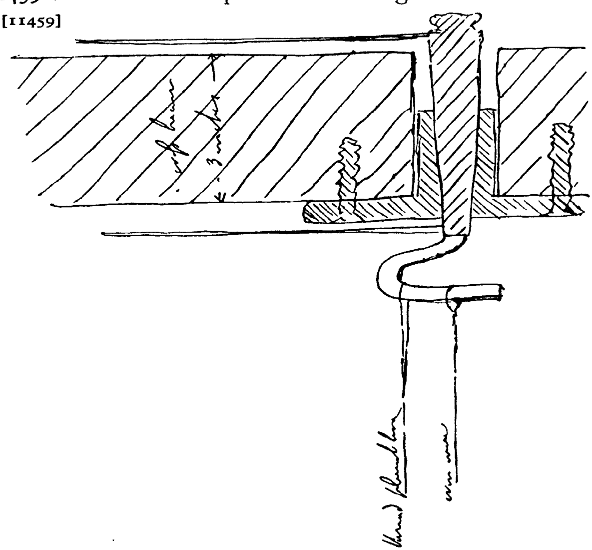
11458. Experimented to ascertain whether a long soft iron wire hung perpendicularly tended to incline towards the direction of the dip. Worked in our lecture room, suspending the wire from a cross bar at the level of the lanthorn light—its lower end being within 8 inches of the lecture table. The wire was freshly, well annealed iron, 27 feet long and $\frac{1}{66}$ of an inch thick; the 27 feet weighed 103 grains. A loop was made on the upper end—the whole length suspended vertically from a smooth round pin fixed above—a weight attached to the lower end and then the wire stretched by a direct pull downwards until it was elongated a couple of feet or more. Whilst the weight continued on, it appeared perfectly straight, but on cutting off the wire 8 or 9 inches from the table below, then it shewed contortions and flexures—little above, for there the weight of the wire pulled them out, but more below, the extreme deflection being perhaps half an inch from the true line. Further pulling, though it lessened the flexures, did not quite remove them so as to give a straight line.

11459*. Above was a plate fixed having a vertical socket and

* [11459]



conical pin moving freely and steadily on a vertical axis; below, the pin terminated in a horizontal support of round wire as in the figure, which could be placed in any direction by means of either of the pins serving as indexes attached above and below to the axis; and by an adjustible plate on the upper surface of the wooden beam, the Magnetic meridian could be marked and also the line perpendicular to it. A thread with a small weight at the bottom was first placed by a loop on the support near up to the bend or concavity, and then the stretched wire was carefully transferred from the stretching pin also on to the support, and as near as might be under the center of motion. This being done, the next object was to place the support truly magnetic east and west, and observe the lower ends of the thread plumb line and of the wire by a telescope looking east and west on a scale behind the projection of the wire and thread; and afterwards to turn the pin round 180° so as to place the points of support again east and west and observe again. The thread bob ought of course in both places to give the same position on the scale. The iron wire because of its contortions ought to give two positions, but the intermediate point would correspond to that of a perfectly straight wire–and would correspond with the thread if the wire hung perpendicularly–or would appear to Magnetic north of the thread if the wire were sensibly deflected towards the dip. The thread bob was easily held on one side (by the end being in a glass) when the wire was wanted alone, and having a little water in the glass when it was brought up into its place, the motions of it could easily be diminished and stopped.

11460. Two chief precautions amongst many others were required: one, that the wire should be so hung on its support that no rotatory motion should be allowed on the support above, but the body of the wire turn round through as many degrees as the axis above (or else because of the flexures great errors appeared); and also that the current of air in the room should cease, and time be allowed for motion and temperature and all other things to equalize and give a steady result.

11461. All proper precautions being taken, there were at first signs of some result, but upon repetition of the observations many times and in two different parts of the room, the conclusion was

that no sensible deflection of the wire (bearing only its own weight) could be observed. Still, the weight of that wire was considerable (103 grains), and the effect would have been very marked if it had appeared.

11462. I examined this thin long wire by a magnetic needle about 4 inches long to see how far it acted by induction, as a poker. It did *not* present distinct traces of a polar condition. Its lower end attracted either end of the magnetic needle and apparently with equal force. Again, a piece of the same wire about 2 feet long was not distinctly magnetic by position in the dip, but when folded up into half and a quarter that length, then it began I think to act. So also a short piece of three inches in length acted.

11463. It was as if the extreme length compared to the thickness was against a general induction of the mass, but must try this again more carefully (   ). It may be that such an effect may be important in relation to the case of atmospheric magnetism, i.e. that lateral extension is needed in such cases as oxygen, etc. etc.

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11464. It is astonishing to observe how difficult it is to obtain a length of this wire entirely free from magnetism for experiments on the induction of the earth. If a piece, being straightened, is cut with scizzors or tools themselves magnetic, it becomes magnetic. If, being in a vertical position, it be cut with tools not magnetic, it becomes magnetic. If a piece be made red hot in a spirit lamp, cooling as it goes out of the flame, it becomes magnetic if the heated end is at all out of the horizontal line. If the wire, being held horizontal, bends during the heating, it comes out certainly magnetic. If the end heated be *retained* horizontal, or rather in a plane perpendicular to the dip during the heating, still if the other end or parts of the wire be raised or lowered above that plane, the heated end becomes magnetic. Almost any heating and cooling, or cutting or jarring action makes the wire magnetic if the *whole* be not in a plane perpendicular to the dip. Hence it requires great care to ensure that any piece intended for experiments of induction by position should be free from magnetism—and whilst in the dip, almost any action, a knock or blow or rub,

will make it magnetic. The iron was very soft—even after ignition and sudden quenching in cold water.

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11465. Made a few more experiments upon friction in the Magnetic field, but with the R or running body (11437) at least ½ an inch thick, to allow of any effect that might be due to the length of the part moving *across* the lines of force and therefore between the fixed S, S.

11466. The *copper* on flannel for S, S (11436): the R was a cylinder of shell lac 0·7 of an inch in diameter. The magnet was the same as before—being the great one urged by 20 pr. Grove's plates. The running shell lac was *Negative* and on both sides—so no effect of the magnetic force sensible here.

11467. *Shell lac* on wood was now made S, S (11436)—and R was a copper cylinder 0·6 of an inch in diameter. The shell lac was now *Positive* and both of them were so—very clearly and strongly—and so the reverse of what they were when shell lac was the runner (11466); but in this case the friction was not smooth but with a sound or rattle or chirp, being an *intermitting friction*—and this was probably the cause of the reversion. The copper cylinder had been turned in a lathe—and when cleaned by the sand paper shewed that it was covered with ring irregularities produced by the tool in the lathe. It had not been finished off by a flat edged tool. These irregularities would tend greatly to produce the chirp friction or intermitting friction.

11468. *Shell lac* still S, S, and R a cylinder of tin 0·6 in diameter; it had been turned in the lathe as the copper and shewed the same marks on cleaning with sand paper. It produced chatter friction and left both the shell lacs *positive*.

11469. *Shell lac* still S, S, and R a cylinder of bismuth—the friction was without chatter and both the shell lacs were *Negative*.

11470. Shell lac S, S. R made tin again—friction with chatter and both shell lacs *positive*.

11471. Now rubbed these different bodies together, being the *same* articles but out of the Magnetic field.

Shell lac and Tin—	no chatter—Lac left Positive.
„ „ copper	Do. . . Do. Do.

Shell lac and bismuth	Do. . .	Do.	Negative strongly
„ „ Antimony	Do. . .	„	Positive
„ „ Fusible metal	Do. . .	„	Negative.

11472. Thus the Magnetic force adds nothing (11475).

11473. But a continuous friction and an intermitting friction produce very different results, at least with *Copper and Shell lac*, for when it is continuous the *shell lac is Negative*, and when it is intermitting the *shell lac is Positive*.

11474. Again, there is an unexpected difference between bismuth and the other metals, for when the friction is smooth, Tin, copper and Antimony left *lac positive*—but bismuth and fusible metal left it *negative*.

11475. Must ascertain however what is meant by the fact that, in the magnetic field, the copper made the running shell lac Negative (11466), though the friction was quiet or continuous—but that out of the Magnetic field it made it Positive (11471). I think the rubbing bodies were the same in both cases (11479).

11476. *Gutta Percha* rubbed against *Bismuth*, *copper*, *Tin*, *Antimony* and *Fusible metal* became Negative in *all cases*—being at the times of the experiments out of the magnetic field.

11477. *Gutta Percha* and shell lac rubbed together, the Gutta Percha was Negative. So that bismuth would seem to come between Gutta Percha and shell lac.

11478. *Copper* on flannel was made S, S (11436) and a plate of *mica* was made runner between, in the magnetic field. The mica did not become sensibly electrified. Perhaps being cold it did not insulate well enough to retain a charge—but there was no effect to encourage a notion that crystalline structure might aid in developing any effect.

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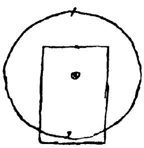
11479. Made further experiments—found at first that shell lac against copper was always left Positive—but then, by varying the rubbing, it was sometimes Pos. and sometimes Negative—though the friction was smooth—and that Magnetism had nothing to do with it was very evident. Careful investigation would no doubt make out the cause of the difference (11475). Abrasion of the shell lac surface has probably an effect.

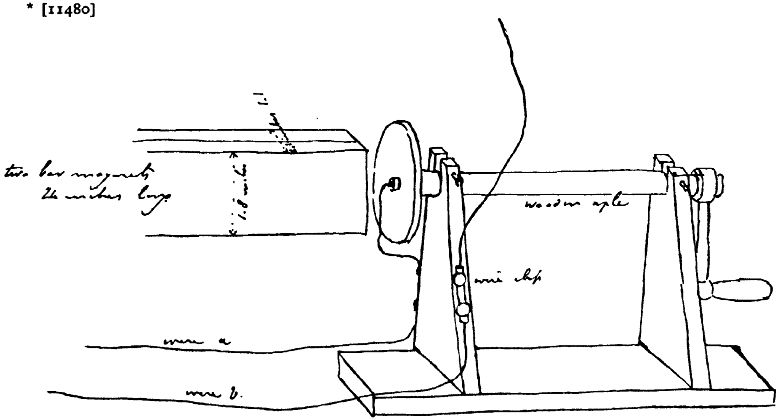
11480*. An apparatus was constructed consisting of a wooden stand and supports carrying a wooden axle which could be turned by a winch. On the end of the axle was fixed a copper termination ending in a male screw, on which different disc[s] of metal could be screwed, so as to be fixed against the copper shoulder and revolve with the axle. The end of the copper screw had a conical aperture turned in it to receive the end of a copper wire *a*, which by its spring and elasticity pressed into it and so made contact there. Then another wire *b* was employed to make contact at the edge of the disc, or any where else upon its cleaned surface where it might be desirable. The wires were held in wire clips fixed to the edges of the support and went away to the Galvanometer at a distance. This disc was intended for revolution within the magnetic field of any magnetic system, and the currents induced in it were to be rendered evident and measured at the Galvanometer. Its diameter was $2\frac{1}{2}$ inches and its thickness 0·1 of an inch.

11481. Employed at first our two large bar magnets with like poles together, so as to make one magnet 24 inches long, 1·8 inches deep and 1·1 inches broad. It was placed end on before the revolving disc at 0·5 of inch distance from it, but was not concentric with it but thus. The one wire, *a*, was always applied at the center of the revolving disc and the other, *b*, at different places on the edge, care being taken by cleaning, pressure, etc. that contact was continuous.



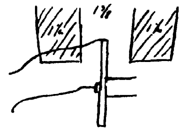
11482. When the wire *b* was applied at 1, and the disc revolved, there was an effect at the galvanometer, but small, only 3° or 4° to the right or left for 20 revolutions, according as the motion was one way or the other. When the wire *b* was applied at 2,

* [11480]

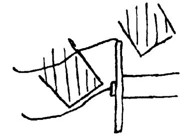


then the effect was greater, being 4° or 5° for the same number of revolutions.

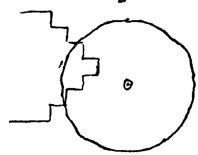
11483. Thus the principle of action was shewn in different points, but the power is not enough to give good and comparative results. The limit is soon obtained and in fact 40 turns here do no more than 20 (11354). So it will be better to use the concentrated field of a horseshoe magnet.



11484*. Employed our larger permanent horse shoe magnet, which has the dimensions marked at the poles and can sustain well 40 lbs. by the keeper and weighs 16 lbs. nearly. When it was arranged as thus in plan, and the wires applied as shewn, ten revolutions of the disc gave a deflection of 6° or more at the galvanometer, and a continuous revolution could sustain a deflection of 18° or 20°.



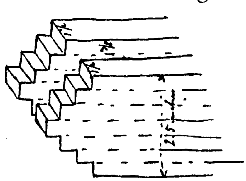
11485. The frame of the stand prevented me from approaching the magnet nearer if at right angles to the axis of rotation. I therefore turned it round thus, but did not obtain a better deflection; and considering that it is the outer end of the moving radius that produces the chief action, a better was not to be expected. So returned to the former position (11484) in which the disc was midway between the two poles, at right angles to the magnetic axis, and the moving radius about half within the face of the poles and half without. The axis of the disc was however below the middle of the magnet (because of the intervention of the frame) about $\frac{3}{8}$ of an inch.



11486. Now the galvanometer wires being applied at the center and at 1, and ten revolutions made by a quick steady motion of the hand, the deviation was 6° nearly in either direction, according to the course of the revolution, and the swing of the needle continued longer than the time of the ten revolutions (indeed nearly half as long again), shewing that the whole effect was included (11354).

11487. Experimented—and wished first to see if a second set of wires from the Galvanometer could carry more power to it from other radii of the disc than that included in the first wire (11404, 5). So had two wires from each of the galvanometer cups or screws; these may be called *a* 1 and *a* 2 for one cup and *b* 1 and *b* 2 for the other cup. They were about ten feet long each and from $\frac{1}{30}$ to $\frac{1}{25}$

* [11484]



of an inch in thickness—were of copper and covered with cotton thread.

11488. *a* 1 and *b* 1 (11487) being applied at the center and inner edge of the revolving disc, then ten revolutions gave a deflection of 6°. Then *a* 2 and *b* 2 wires were applied in the same way and gave a deflection of 6° also. On applying both *a* 1 and *a* 2 to the center at once and *b* 1 and *b* 2 near together to the circumference at once, a deflection was obtained perhaps a little more than 6° but not more than 7°.

11489. Then obtained results from the different sets of wires one after the other, intermingling them, so as to procure a better average as regarded inequalities of pressure, etc. The following are the number[s]:

With wires	*a* 1 and *b* 1 . .	$5\frac{1}{3}$°, 5°·5, 6°, 6°·2 . . .	average 5°·76
„ „	*a* 2 and *b* 2 . .	6°, 6°·2, 6°·2, 6°·2 . . .	average 6°·15
„ „	{*a* 1 and *b* 1, *a* 2 and *b* 2} . .	6°·2, 6°, 6° . . .	average 6°·06

So the second wires here added nothing sensibly to the effect of the first (11484, 90), and were in their own effect diminished by the addition of the first. The wire *b* 2 was thicker than any of the others and that I believe made that pair superior in their particular effect.

11490. The Galvanometer wire being thin and long is no doubt a great retardation to the current generated in the moving radius, of which the whole length is only 1·25 inches, and the effectual or chief part not more than $\frac{1}{3}$ or $\frac{1}{4}$ of this (11492), and it is this which limits the effect on the needle. The current in *a* 1 and *b* 1, when it arrives at the Galvanometer screws or cups, finds there both the galvanometer wire and also the wires *b* 1 and *b* 2 through which it may direct its course or expend its power, and though the latter has an opposing current in it, it is by far the most favourable and best conductor of the two; and so part of the force in wires *a* 1 and *b* 1 are retained by and expended in it, the rest going on to the Galvanometer. The wires *b* 1 and *b* 2 exert a corresponding action on the current in *a* 1 and *a* 2, and thus only a part of its power goes on to the Galvanometer. So when the two sets of wires are used, the currents in each oppose in part that in the other, and the resultant of force only goes on.

The opposition really is between the two moving radii opposed by the wires to each other, for there the forces are initiated and there the contrary force is prevented in part from passing, or returning (11404, 5, 11494).

11491. If both the sets of wires were continuous round the galvanometer, then both would shew their full effect either separately or together. The resistance of the fine wire in the galvanometer is the barrier to accumulation of two or more currents from different radii when of such very low intensity. So a Galvanometer with a thick wire and few convolutions may be far better than one with a long thin wire and many convolutions (11525).

11492. Experimented as to the efficacy of different parts of the radius. It is evident that the part near the center moves with a less velocity than that near the circumference, in proportion indeed to the distance from the center—and that therefore the part near the circumference is the most effectual. Also that that part is a place of more intensity as regards the magnetic forces (11485) because of the position of the disc and magnet. So making contact of *a* at the center always (11486) and of *b* at different distances from it towards the circumference, the following were the results with ten revolutions in every case. It made no difference on which *side* of the revolving disc the second wire *b* was applied.

The central $\frac{1}{2}$ radius gave $2^\circ{\cdot}5$, $2^\circ{\cdot}5$, $2^\circ{\cdot}5$ deflection.
The central $\frac{2}{3}$ radius gave 3°, 3°.
The whole radius gave 6°, 6°.
The circumferential $\frac{1}{2}$ radius gave 5°, $5\frac{1}{2}^\circ$.
The circumferential $\frac{1}{5}$ of radius gave 3°, $3\frac{1}{2}^\circ$.

All this shews how powerful the outer end of the radius is, and that when the moving part was only $\frac{1}{4}$ of an inch in length and had all the lateral parts of the disc to discharge power more or less by, backwards, still it could send a current through all the resistance of the Galvanometer able to deflect the needle $3^\circ{\cdot}5$. It gives great hopes for the use of a revolving ring apparatus (11424).

11493. Where the outer parts of radii were used (11492), of course the wire *a* was not at the center of the disc but on the face. If the wire *a* were carried to the edge of the disc and the wire *b* brought within the circumference towards the center, then

with the same direction of revolution the deflection at the galvanometer was reversed.

11494. I have *three* discs of copper, the same diameter of 2·5 inches but of different thickness, one being 0·05, a second 0·10 and the third 0·2 of an inch in thickness. They all gave the same deviation—a result to be expected from the experiment with two sets of wires—for it is only that experiment in another form, i.e. within the body of the disc itself (11487).

11495. Proceeded to use other substances than copper for the revolving disc. A *bismuth* disc $2\frac{1}{2}$ inches diameter and 0·2 of an inch thick was first employed. The deflection of the needle was very great, far more than enough to make it strike against the stop—but it was the same whichever way the disc revolved and had nothing to do with the magnet, being due to the friction and heat and thermo current of the copper wire rubbing against the edge of the disc.

11496. So the copper wire *b* was made fast to a rod of bismuth, and being held in a cloth so as to communicate no difference of temperature, the bismuth rod was held against the edge of the bismuth disc. Still there was great deflection (60° or 70° for ten revolutions) due to the friction—it was always in the same direction and was probably due to the thermo current produced by the difference in temperature of the point rubbed of the immoveable rod and that of the moving circumference of the disc. It is too much to manage and makes this form of experiment with bismuth useless.

11497. Would require a bismuth roller on the end of the wire *b*, or even a disc as large as the chief one to avoid difference of temperature. But probably the thick wire galvanometer (11525) and a rod of bismuth fixed at both ends may do.

11498. A disc of *Steel*, not hardened, was made and used; it was 2·5 inches in diameter and 0·2 of an inch in thickness. Ten revolutions in the same direction gave at different times 4°, 4°·5, 4°, 4°, 4°. Ten revolutions in the contrary direction gave a contrary deflection also but to a much larger extent, namely 10°, 10°, 10°·5, 10°, 9°·5, 10°·3.

11499. So here we see at one time the *difference* of the effect of the magnet and of the edge friction of steel and copper wire

(11495), and find it to be in favour of the magnet effect, the result being a deflection of 4°·1; and at another time the sum of the magnet and friction effect, the deflection being then 10°·05. Half the difference must be nearly the effect of the friction, i.e. 2°·97 or 3°. In that case the magnetic effect of 10 revolutions of steel would be 7° or 7°·1, which is more than that with copper, and that is quite consistent with the effect that the steel would have of drawing force upon it and increasing the intensity of the lines of force passing through it whilst it was there.

11500. *Soft iron disc*, 2½ inches diameter by 0·2 of inch thick. Ten revolutions in one direction gave deflections of 2°·5, 2°·5, 3°, 2°, 4°, 3°, 4°, 4°. Ten revolutions in the reverse direction gave 10°, 10°·5, 10°·75, 10°·75. So the difference of magnetic and friction action was 3°·125 of deflection in one direction—and the sum was 10°·5 deflection in the other direction. Half the difference is 3°·6875 or 3°·7 nearly, so that the magnetic effect of revolving soft iron is 6°·8 nearly, or rather less in these expts. than that of steel. The effect of friction appears to be more than with steel, perhaps because it is softer and so the friction really greater.

11501. Then removed the magnet to ascertain experimentally the effect of the friction by itself. Whichever way the rotation of the disc was effected, the needle deflection was the *same* and unaltered; with the direct revolution the deflections were 4°·5, 4, 5°·4, and with the reverse revolution 4°, 4°·5, 4°. The average is 4°·4, which is greater than that above. Being given from Zero position, it should appear in figures greater, but all the experiments are to be considered as approximations only.

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11502. The copper disc of 2½ inches diameter and 0·1 thick was on the axis and between the magnet poles as before (11485). The pressure of the wire *b* against the edge of the disc at 1 was made by applying the finger on it, which would of course somewhat warm it. One direction of rotation gave for 10 revolutions 5°, 4°·5, 4°·5, 5°, 5°·5, or an average of 4°·9. The other direction of rotation gave the contrary deflection of 6°·5, 6°·5, 6°·5, 6°·5, 6°·5, giving an average of 6°·5. When the magnet was away, there was a small deflection during rotation (and friction at the edge) but

always the same way for both direction[s] of rotation and coinciding with the increase and diminution of the former averages. It was 1°·5, 1°, 1°·2, 1°, 1°, giving an average of 1°·14. The effect of friction deduced by halving the difference of 6°·5 and 4°·9 would be 0°·8. The former is obtained close on the normal line and the latter at the end of an arc of 5° or 6°, and therefore the latter ought to appear less for the same amount of force.

11503. As the effect of friction is added on one side and subtracted on the other, it creates no difference in the sum of the deviations (only throwing the mean position on one or the other side of Zero); so adding 4°·9 and 6°·5 and halving the sum gives 5°·7 as the deflection either on one side or the other, due to the current produced by 10 revolutions of the copper disc. The former results were 5·76, 6·15 (11489).

11504. Friction by pressure of the finger should be avoided (11502).

11505. *Iron (soft) disc* (11500) of 2½ inches diameter and 0·1 of inch thick. The magnet was not applied, the object being to ascertain what degree of current different degrees of pressure, etc. exerted in bringing the copper wire *b* against the edge would produce. Found that a gentle spring pressure produced 1°·0, 1°·5, 1°·2 of deflection always in the same direction—the average being 1°·23. When the copper wire was pressed strongly by a piece of wood, so that no warmth from the hand was communicated, then the deflection[s] (in the same direction) were 4°, 5°, 5°·2, or an average of 4°·73. When the pressure was given with the warm finger strongly against the flat edge of the disc, the deflections were 7°, 6°, 7°·5—or an average of 6°·83. When it was made with the warm finger strongly against the very edge of the disc, so as to rub the copper only at one point, the deflection rose as high as 11°. Shewing how much it is exalted by different circumstances, and what care should be taken to make it the smallest necessary degree and always uniform in the circumstances.

11506. Now the wire *b* was terminated by a foot in length of strong *iron* wire, to rub against the iron disc. All was left a while to attain the same temperature. After a while, holding the iron wire so as not to alter its temperature and applying it for the

time of ten revolutions, but without revolution, to the edge of the disc, it appeared to produce about 0°·5 of deflection.

11507. Then gave 10 revolutions, the iron wire meantime pressing on the edge by a gentle spring effort—quite enough to give contact for any other current induced or otherwise excited. This revolution scarcely added anything to the effect. So that such contact is good for an iron disc (11511).

11508. Ten revolutions during cold hard pressure with wood (11505) gave a deflection in the opposite direction to that produced by the copper wire (11505): the results were 6°, 6°, 7°·5, or an average of 6°·5, which is much more than for the copper wire and cold pressure (11505).

11509. Ten revolutions with hard warm finger pressure (11502) gave 6°, 4°, 4°, 5°, or an average of 4°·75. I think the wood gave a harder or at least a more cutting pressure than the finger; when the actions were alternated, there was more roughness in the friction under wood pressure.

11510. Warming the iron wire in the hand and then applying it to the edge of the disc without revolution produced about 2° of deflection in the same direction as with friction.

11511. The *best* condition of friction (11507) of the cold iron wire against the cold disc gave about 1° of deflection.

11512. Before obtaining the last results I had, by finger pressure of copper wire against the iron disc and without the magnet, had the following high number[s]—rubbing sometimes against the flat edge and at other times at the angle: 9°, 7°, 8°·5, 7°, 7°, 7°, 6°·8, 8°, 9°, the average being 7°·7.

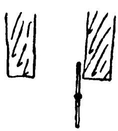
11513. Now put the magnet in place (11485) and gave pressure of finger. The deflection in one direction was 14°·5, 14°·6, 14°·7, average 14°·6. In the other direction the deflection was 3°·5, 3°, 2°·5, 3°, 1°·5, average 2°·7. $\frac{14^\circ\cdot6 + 2^\circ\cdot7}{2}$ gives 8°·65 as the average deflection of the current produced in the revolving iron disc.

11514. With a less but uncertain pressure at different times—the following results were obtained. In one direction, 7°, 7°·6, 7°·8, aver. 7°·5. In the other direction 6°·5, 7°, 6°·5, 7°, average 6°·75. $\frac{7\cdot5 + 6\cdot75}{2}$ gives 7°·125 as the induced current in the iron disc. Both this number and the 8°·65 require correcting by more careful

experiments. These results were obtained before those on the preceeding page[1], and I was *not* at the time fully aware of all the precautions.

11515. *Steel disc*—not hardened, 2½ inches diameter and 0·1 of inch thick. Without the magnet and with pressure of finger directly on the copper wire against the edge, the deflection at the galvanometer was in the same direction as for iron, i.e. copper wire on iron—but the reverse of that for copper wire on copper. The amounts were 6°, 6°, 6°·4, 6°·3, 7° (rubbing at the edge)—the average, 6°·34.

11516. Then with the magnet in place (11485) and disc midway as near as might be, deflection in one direction was 14°, 14°·5, 14°·3, average 14°·3; in the other was 2°, 2°·5, 2°, average 2·2. $\frac{14{\cdot}3+2{\cdot}2}{2}$ gives 8·25 as the deflection by the current produced in steel, which is a little less than that produced in iron, 8°·65 (11514). The effect of friction deduced by halving the difference of 14°·3 and 2·2 is 6°·05, which is not very different to 6°·34 deduced directly.



11517. To ascertain the effect of position of the steel plate, I removed it from the middle to one side, near to but not touching the inside of the magnet. Now the rotation in one direction produced 20° and in the other 6°, giving a mean of 13° instead of 8·25, and shewing how much more powerful the curves in it were and how much position influenced a steel plate.

11518. *Copper and Iron* at once. The copper disc 0·1 thick was placed on the axle, then a disc of paper and then the iron disc of 0·2 in diameter. Without the magnet, the copper wire *b* applied gently to the edge of the copper disc gave a mere trace of friction effect at the galvanometer. Being applied in the same manner to the edge of the iron disc, it gave 1°·5 nearly of deflection to the right.

11519. Now adjusted the magnet in place (11485) so that the discs were jointly midway between the poles and the iron therefore nearer to one side than the other. By applying wire *b* to the edge of the copper gently, the effects were in one direction 6°·2, 6°, 6°·2, 6°·2, 6°, 5°·8, 6°, 6°·2, 6°·2, giving an average of

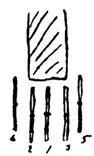
[1] I.e. pars. 11506–9.

6°·1. In the other direction the deflections were 7°, 7°, 6°·5, 6°·5, 7°, 7°, 6°·8, 6°, 6°·8, 6°·5, 6°·8, 6°·7, the average being 6°·76; so the mean deflection of the copper associated with the iron is 6°·43.

11520. The wire *b* being now applied to the edge of the iron disc gave the deflection on one side 8°, 7°·5, 8°·1, 7°·5, 8°, 8°, 6°·2, 6°·2, 8°, 8°, 7°·5, 7°·5—the average, 7°·54. On the other side the deflections were 5°·5, 6°·2, 6°·1, 5°·5, 6°, 5°·5, 6°·5, 7°·0, 6°·2, 6°·2, the average being 6°·07—the mean of the two is 6·8 for the iron associated with the copper. The high number[s] 8°, 8°, were when the wire *b* was at the sharp edge of the disc.

11521. Iron therefore presents more intense action than the Copper of the same size and in the same field; but considering the effect and thickness of the iron, this ought to be so, for the iron would cause lines of force to bend down upon and through it, which when they came to the copper would pass up and out of it, escaping it. By fastening a small needle ⅛ of inch long transversely on a long thread and stretching the thread so that the needle could be introduced between the poles and used to examine the direction of the forces there, it shewed a short sharp deflection of the lines of force into the iron at the edges of the disc—as figured above*; and so justified the conclusion—for that is the most effective part of the moving disc.

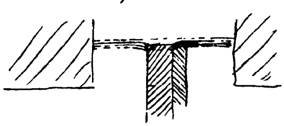
11522. Motion in the plane of the lines has no tendency to produce a current. The copper disc was placed edge on to the end of the bar magnet and in other positions, and then revolved, the galvanometer wires being applied at the axis and edge nearest the magnet as before. In position 1 there was no effect. In positions 2 and 4, currents were produced in one direction—in positions 3 and 5 currents in the other direction, with the same revolution of the disc. This shews that in the plane of the line of force there is no effect.



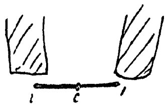
11523. So also when the disc was placed thus† in the very strong field of the horseshoe magnet, rotation produced no effect.

11524. When the disc was adjusted at the horse shoe magnet so

* [11521]



† [11523]



as to be equally subject to both poles, and the wire *b* applied at the edge over *c* so as to be equidistant from the two poles, then there was no deflection. If applied at 1, there was deflection one way equal to 4° or 5°, and if applied at 2, deflection the other way to the same amount. If the two wires were applied at 1 and 2, there was a deflection of 8° or 9°—or sum of the two former—as should of course be the case.

11525. *Thick wire Galvanometer* (11490). It must be better to have a short thick wire for a Galvanometer with currents of feeble intensity, and then other wires (thinner than it) can be carried close up to it. So had a rough one made as in the following figure*, with copper wire 0·2 of an inch in diameter. The needles were a pair belonging to Rhumkorf's[1] Galvanometer and 1·85 inches in length—touched and adjusted so as to make a vibration in one direction in 19 beats of my watch. The whole was covered with a glass shade, being very rough in construction, and as yet has no graduation. After finishing the ensuing experiments of this date, a card circle of graduation was applied to it.

11526. The whole length of the thick wire AB was 19 feet 4 inches—the ends were soldered together.

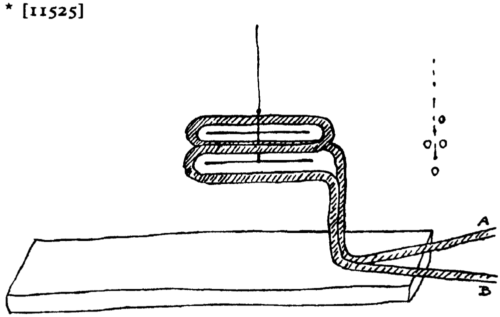
11527. The horse shoe magnet (11484) was set upright upon the table six feet from this galvanometer and the loop or continuation of the thick wire passed once through between the poles and left at the equator. This produced a deflection of above 90°. When taken out again there was corresponding large deflection on the other side. So the Galvanometer is abundantly sensible and far surpasses that with many coils of thin wire for these currents of low intensity, though here there were only two convolutions equivalent to a figure of 8 (11706).

11528. As to quickness of the motion of the wire—when quickest is best. Still, several different degrees of quickness, being all pretty high, gave 135° or 140° of deflection; whilst a slow motion gave only 45° or 50°—and a very slow one still less.

11529. As to this difference, the reason before spoken of (11354) is one, namely, that the current of low intensity is opposed by the earth's force more effectually than that of high intensity. But

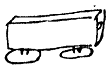
[1] Faraday's spelling of Ruhmkorff.

* [11525]



there is another, very sensible when the deflection exceeds 5°, 10° or 15°, namely, that as the needle departs from parallelism with the wire and currents, it is less affected by them though of the same strength. If at right angles, it would not be affected at all. So when the current is over before the needle has much departed from its first position, the force on it is greatest and the swing due to momentum shews the rest. All this will do for general comparison of one degree with another, but not for measurement.

11530. Retarding effect of a contact or of a thin wire. Cut the wire across, cleaned the ends well, placed them together by their round sides, pressed them together and then passed the loop between the poles. If the pressure at the joint was good, the effect at the galvanometer was apparently as good as before, but if it were weak or any dust or film of oxide there, then there was less swing from the obstructed passage. When the two ends were clipped hard in the holding screws, then the contact appeared to be very good. But no doubt perfect continuity is best—next to that soldering—then binding well with cleaned wire—then the screw clips—and last pressure or holding.



11531. Made a length of copper wire, $\frac{1}{22}$ of inch in diameter and 28 inches long, a part of the circuit, binding it well on to the ends of the thick wire. Passed the thick part of the loop through the poles of the magnet, but now the quickest motion could not produce a swing of more than 45°, so great was the obstruction caused by this wire. Took it out and replaced it by 19·5 inches of other copper wire $\frac{1}{74}$ of an inch in diameter, and now the utmost deflection was only 7° or 8°, so great was the obstruction; and by proof of the experiment so good and valuable the thicker wire.

11532. Restored the circuit until all thick wire 0·2 of inch diameter, and left the loop in the bend of the magnet, i.e. at the equator. When the galvanometer was at rest, the mere putting on or removal of the keeper or submagnet caused a considerable deflection, i.e. several degrees in the two opposite directions.

11533. As to the question of whether, when the magnetic poles are brought near each other, there is more development of external magnetic influence, or only a more concentrated arrangement of it, made the following experiments with this sensible galvanometer and the continuous thick wire circuit. The wire loop was

fixed between the poles—its place is shewn in the section*—then a cube of soft iron was occasionally placed as in the figure and the effect of its presence observed. When the iron (being away) was put into place, whether from above or below, it caused a deflection of 3° or 4° in one direction, being the same whichever way the iron was carried to the magnetic axis. When the iron cube was removed, it caused an equal deflection in the opposite direction. When the iron cube was carried into and out of its place in one motion, then the sum of action was nothing—there was no deflection (11537[1], 11570).

11534. This effect cannot shew however whether the presence of the iron increases the whole amount of external force or whether it only displaces it. So passed the wire across the magnetic axis, well through all the curves, i.e. from a distance outside of a foot through to the magnetic equator, the iron cube (11533) being sometimes in the magnetic axis and sometimes away; but did not perceive any sensible difference in the amount of action in the one case or the other. So the amount of magnetic force in the external lines of force appears to be unchanged.

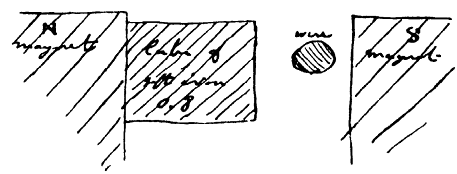
11535. With the present position of the magnet and Galvanometer, the needle of the latter, when deflected by hand to a right angle with its natural direction, took 19 beats of my watch to pass when left in one direction, and this time it took for the four or five following swings. Then brought the magnet to within half the distance from the galvanometer and still the times were as nearly as may be 19 beats, perhaps a little less—but for the present I may consider the Galvanometer as undisturbed by the nearness of the magnet.

11536. Removed ten feet of the wire, leaving between 9 and 10 feet connected at the ends by a screw (11530). Now when the wire was passed between the poles across the magnetic field, the needles revolved three or four times and shewed that a more powerful current passed than before, because the resistance was less by shortening the wire.

11537. As the resistance is directly as the length of the wire, it would not be difficult to graduate the instrument as to the extent of swing by taking wires which, by variation of length only,

[1] ? 11538.

* [11533]



should give resistances in regular degrees, and observing to what extent the needles were thrown, upon the same evolution of current or the same tendency to evolve current.

11538. Repeating the experiments with the cube of iron, the results were as before (11533) and the effects but small.

11539. To compare the effect of this horse shoe magnet with the former bar magnets (11322) used in the revolution experiments, I made a loop in the thick wire the whole length of which was now about 16 feet, and putting the two bars (11322) with like ends together, I passed them through the loop until the latter coincided with the equator. The needle moved through 30°, shewing a very good effect. The motion of the loop outside the bars or the bars outside the loop did nothing. So that this galvanometer is better for those expts. than the former—and in the case of one or many wires, will tell what that could not (11385, etc.).

11540. I now repeated some of the disc experiments (11494, etc.) with this galvanometer having thick copper wires up to the axis and the edge of the disc. Found that ten revolutions could not be permitted. Three revolutions were enough to deflect the needles 40° or more (11610).

11541. Employed the three copper discs of $\frac{1}{20}$, $\frac{2}{20}$, $\frac{4}{20}$ of an inch in thickness (11494), but they appeared to give nearly all the same result, about 40°. I could not perceive any clear difference. Perhaps the side metal of the discs returns the extra current generated in the extra thickness and so it will only be in very thin wires that comparisons of this kind can be made (11615).

11542. The *soft iron disc* (11500) $\frac{1}{10}$ thick, $2\frac{1}{2}$ inches diameter, with soft pressure of copper wire at the edge, when between the magnet poles gave for three revolutions less than any of the copper discs. With increased pressure it could be made to give as much or more in one direction of rotation, but then less in the other. For without the magnet and with pressure and rotation, it gave deflection in the increased direction (11616).

11543. The steel disc (11498) without the magnet, with a soft pressure of the copper wire at the edge, very little deflection. With a hard pressure, 20° or more.

11544. Then put up the magnet with soft pressure; deflection not more than 10°; and when rotation reversed, about as much

the other way. Then gave more pressure and the reverse rotation, and the deflection about 10° only *accordant with the rotation.* Now much pressure and *direct rotation*, and the needle swung to 90° or more direct for the three revolutions. So here effect of friction action high.

11545. Put on the 0·1 copper disc again. Without the magnet, whether the pressure of the copper wire on the edge of the disc was slight or considerable, there was no sensible evolution or current at the Galvanometer. Then put up magnet, and found that it wants a full pressure to get full effect of the current—a weak pressure lets some escape.

11546. Some other experiments with attention to pressure seemed to bring iron up to copper, but steel apparently was not so high; but the comparison is uncertain.

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11547. Have been preparing certain rings of covered wire containing different lengths of wire of different thickness, made up into rings of different diameter—for rotation in lines of force. They are numbered and the following are the data for each:

No.	Substance	Diamr. of wire	Inches in the ring	Spirals	Diameter of ring	
I	copper	·008	63	20	1·0	Waxed outside
II	copper	·024	40	12	1·0	Do.
III	iron	·018	197	31	2·0	Do.
IV	copper	·032	49	10	1·5	Do.
V	copper	·038	120	19	2·0	Do.
VI	copper	·025	650	64	3·3	Covered with silk ribbon outside.
VII	copper	·040	300	26	3·6	Do.

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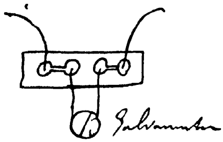
11548. The temporary thick wire Galvr. (11525) in use to-day. The silk suspension is about 5 inches long and of one fibre. 2 turns of it sets the needles considerably this or that way,

[1] Here and at two other places in the MS. Faraday has written "Decr.", evidently a slip for "Septr."

perhaps 6° or 7°. Shewing the little influence of the earth and the great apparent effect of torsion.



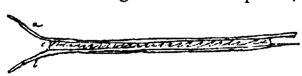
11549. The ends of the Galvanometer wire dip into two cups of mercury which connect with two other cups of mercury intended to receive the ends of moving connexions: the four cups were cut in one piece of wood. All the ends of the copper wires were tinned for the mercury contact.

11550. *Earth's lines of force.* 7 feet of the copper wire 0·2 of inch in diameter were bent into a rough ring and the ends inserted into the outer mercury cups (11549). A motion of the upper part across the line of the dip through about 90° gave a good deflection at the Galvanometer. Two inches each way in harmony with the swing of the needle gave 30° or more of deflection. So this kind of Galvanometer good for magneto electric evolutions by the earth.

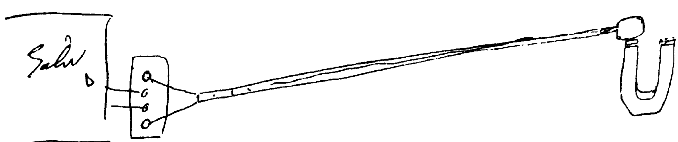
11551*. Prepared sets of good conductors, thus: *a*, *b* are two wires of copper 0·2 of inch in diameter and 35 inches long, tinned at both ends. *c* is a square rod of wood to which they are tied on opposite sides. The ends *a*, *b* are bent down a little and are to dip into the outer cups of the connecting piece (11549); the other ends are to be soldered to rings and other matters to be subjected to the magnet.

11552†. Lengths of copper and other wire, 10·5 inches each and of different diameters, were tinned at each end, and then bent into loops or rings thus, so that they might pass freely over one pole of the horse shoe magnet (11484). They were then soldered each to a pair of the conductors (11551)‡, so as to form like systems except in the diameter and matter of the terminal loop. Contact across at *a* was prevented either by opening the wire or putting a piece of card between. Because of the closeness of the two conducting wires all the way up to *a*, no motion of them in their arranged position (11552) caused any effect at the galvanometer, so the effect of the ring was had pure. On passing from a distance of 12 inches above the magnet over the pole (i.e. one pole) to

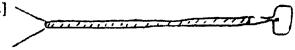
* [11551]



† [11552]



‡ [11552]

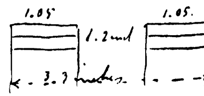


the equator, nearly all the curves or lines of force were intersected once: a little motion at the equator or a good deal when 12 inches off gave no sensible effect at the Galvanometer. The form of the ring would of course make no difference.

11553. Several of these loops on conductors were prepared—all alike in length but of the following diameters:

No. I	copper	0·2	of inch	in diameter					
II	"	0·11	"	"					
III	"	0·0476	"	"	or $\frac{1}{21}$				
IV	"		"	"	or $\frac{1}{21}$	containing	4 wires	side	by side.
V	"	0·0125	"	"	or $\frac{1}{80}$	"	76 wires	"	"
VI	"	0·0476	"	"	or $\frac{1}{21}$	"	2 wires	"	"

11554. The loop No. I (of 0·2 diameter copper) being once passed over one of the poles of the horse shoe magnet (11484), was found to be far too powerful. So a smaller magnet, like it in form and consisting only of five plates, was employed. It weighed 8 lbs. nearly, and could easily sustain 21 lbs. at the keeper. The thickness at the poles and the interval there was thus:



11555. With this smaller magnet, the loop No. I, passed once *in* or *out*, sent the needle twice round: so here power enough.

11556. With the loop No. II (copper 0·11 in diameter and so little more than a fourth of the substance of the former wire), there was less action at the galvanometer—but I think more than one fourth, for the needle swung once round. Still it passed the 180° and went on to a full revolution and vibration.

11557. With loop No. III, copper 0·0476, sectional area $\frac{1}{4}$ of No. II or $\frac{1}{16}$ of No. I, the needle went with the same degree of motion as before, about 80° of swing to right or left of Zero. Now this again I think is more than one fourth of the No. II effect or $\frac{1}{16}$ of No. I effect. This increased effect in proportion to the thinness of the ring is doubtless due to the goodness of the chief conductor remaining the same for all, and so giving better proportionate conducting power for the thin loop current than the thick one. Each loop ought to have had wire of its own diameter to and about the needle to give proportionate effects.

11558. With loops No. II and III (11553) observed the effect of quick and slow passages. Quick passages gave the largest effect. Could be so slow as to give very little effect. Had at one

time a notion that the maximum effect was at a quick motion, but not at the quickest; but believe that the quickest is as good as any and that there is little difference amongst the quickest that the hand can make.

11559. Loop No. IV used, being 4 wires of 0·0476 diameter and copper. In mass therefore it is nearly as No. II or at least approaching it, 4 : 4·8. It gave a very good effect, just swinging the needle once round as II did.

11560. In order to have an idea how much the lateral parts of a disc of copper or lateral wires conducted back the current, I turned one of the four wires back so that it should not pass over the pole but still be a conductor conjointly with the Galvanometer wire. Now it only swung through 100° or 110° instead of to 180° and going round, so much did the one wire bent back conduct; and to prove that, this wire was cut across; then the remaining three passed as before sent the needle round the whole circle.

11561. Loop No. VI, being 2 wires copper of 0·0476 diameter—the deflection was about 12 points or 135° on side.

11562. Repeated the experiments with the presence of the iron cube (11533) with the smaller horse shoe magnet and the thick wire ring No. I of 0·2 diameter. The results were the same as before. Putting the iron cube into its place when the ring was across the magnetic axis produced an effect one way, and removing it, an effect the other way. But when the wire loop was passed across the field, there was not more power than if the cube was away. A thinner loop gave the same result.

11563. Bismuth blocks were employed in the same manner, but then no effect of any kind could be produced.

11564. When a loop is passed slowly upwards from the equator or bottom of the magnet to a foot or so above it—or in the reverse direction—it is very interesting to see how greatly the lines of force are concentrated about the magnetic axis. And also how the introduction of the iron cube increases this concentration very much.

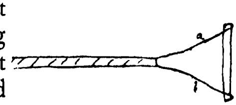
11565. In this way the moving wire gives a manifest and good proof of the removal of the force from one region to another, speaking of the different parts of the magnetic field, when only air is there; and it is easy by arrangement to make the forces

increase in one part and diminish in another and vice versa. Even the change when the wire is stationary and the iron cube is put into place or taken away (11533, 62) is a proof of this.

11566. If a smaller magnetic power be wanted with the same arrangement and distances, it is easy to adjust it by putting the keeper on the side of the magnet across from one arm to the other and more or less near to the poles. The arrangement is very convenient if a stop be placed so as to bring the moving wire to a stand always at the same place.

11567. When the keeper was thus on about half way down the small horse shoe magnet (11554), the thick wire loop No. I of 0·2 diameter gave a deflection of 8 points or 92°. When the loop No. II of 0·11 diameter was used, the deflection was $5\frac{1}{2}$ points or 62°.

11568. To proceed to larger masses than any of the former and in respect of which the conducting wires themselves were small (11602), I procured two copper cylinders each 5·5 inches long, one of which was 0·7 of an inch in diameter and the other only 0·2, so that the sectional area of the first was above 12 times that of the second. These were soldered on to the ends of conducting wires of the same thickness and length as before (11551), but opened out at the magnet end, so that when the cylinders passed between the poles the parts of the conducting wires *a*, *b*, would intersect some of the lines of force. In sum, however, these would be alike in both cases and but few compared to those accumulated between the poles of the magnet. Using the thick wire first, the magnet was reduced in power by the keeper (11566) until the deviation was about 7 points or 80°. After this, whether the thick or the thin cylinder was used, the effect appeared to be the same. Sometimes I thought it was even a little less with the thick cylinder; and this may be, for when it passes, whilst the one part is in the maximum place of action, there are other parts in places of less action, which may return part of the current in the manner of the outer wire before described (11560).



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11569. The needles of the thick wire galvanometer swing in one direction in 20 or 19 beats of my watch. A Magnet brought

near to alter the position of the neutral line lessens the time of vibration, by increasing the force, and so causes irregularity when times are compared one to another. The cocoon fibre should also be *without torsion*, for if that is on, the needle can swing in one direction more easily than the other, because the line of direction of the earth's force is then on one side of the position of the needle; otherwise even small vibrations on each side of the position of the needle are of different extent for the same amount of power exerted.

11570. Repeated the expts. with the cube of iron and found them to be as described (11533). The width of the cube made it difficult to ascertain the minute action when it was in the field because the two edges acted as two angles. To obtain very precise results, the wire ought to be carefully placed and a wedge of iron or a short cylinder be used.

11571. Loops (11552) of *different metals*. Loops like those described were prepared, each on its own like conducting rod. They were of the following metals, all being of one diameter, 0·04 of an inch, and the wire 10·5 inches long. The large horse shoe magnet was used and no keeper (11484, 11566). Copper, Silver, Zinc, Tin, Iron, Platina, Lead.

11572. *Copper* loop (0·04 diameter). The deflections were in all cases observed both in passing in between the poles and in passing out; but as my present graduation is a compass card, the points (each equal to 11·25 degrees) were observed and not the degrees—the results were as follows, expressed in the amount of deviation swing.

11573.

Copper	out	5·8	5·75	6·25	5·75	6	average 5·91	5·6 = 63°
	in	5·0	5·25	6·0	5·0	5·25	,, 5·3	

11574.

Silver	out	5·5	5·5	5·4	,, 5·5	5·5 = 61°·9
	in	5·6	5·5	5·4	,, 5·5	

11575.

Zinc	out	2·8	2·7	2·8	,, 2·8	2·8 = 31°·5
	in	2·7	3·0	2·7	,, 2·8	

11576.

Tin	out	2·0	1·6	1·7	,, 1·77	1·7 = 19°·1
	in	1·5	1·7	1·7	,, 1·63	

11577.

Iron	out	1·75	1·8	1·4	1·5		average	1·61	1·6 = 18°
	in	1·9	1·6	1·4	1·5		,,	1·6	

11578.

Platina	out	2	1·9	1·6	1·7	1·6	,,	1·6	1·5 = 16°·9
	in	1·25	1·25	1·5	1·5	1·6	,,	1·42	

11579.

Lead	out	1·0	1·1	1·1			,,	1·1	1·08 = 12°·15
	in	0·9	1·2	1·1			,,	1·06	

11580. Thus the power of evolving a current appears to vary only with the conducting power of the metal and to have no relation to its magnetic or diamagnetic condition.

11581. The loop V. (11553) was experimented with as above, but wires were gradually cut away so as to reduce their number, that the difference of effect with different masses might be observed. The loop contained 76 wires of 0·0125 diameter and 10·5 inches long.

The 76 wires swung the needle freely round.
66 ,, weaker, but swung the needle round.
56 ,, weaker, but round by 180°.
46 ,, weaker—round in one direction; not in the other (11569).
36 ,, still weaker—but round on one side.

The 26 wires	in	10 points			average	10·5	= 118°
	out	11 ,,					
16 wires	in	7	6·5	7	,,	6·83	7·16 = 80°·5
	out	7·25	7·75		,,	7·5	
12 wires	in	5·25	5·5	5·25	,,	5·33	5·75 = 65°·1
	out	6	6·25	6·25	,,	6·166	
8 wires	in	4·5	4·5	4·1	,,	4·36	4·46 = 50°·1
	out	4·5	4·7	4·5	,,	4·56	
6 wires	in	3·5	3·2	3·2	,,	3·3	3·36 = 37°·8
	out	3·4	3·5	3·4	,,	3·43	
5 wires	in	3	3	3·1	,,	3·03	3·06 = 34°·4
	out	3·1	3·1	3·1	,,	3·1	
4 wires	in	2·1	2·4	2·5	,,	2·33	2·48 = 27°·9
	out	2·6	2·6	2·7	,,	2·63	
3 wires	in	1·8	1·8	2·0	,,	1·86	1·94 = 21·8
	out	2·0	2·1	2·0	,,	2·03	
2 wires	in	1·2	1·3	1·4	,,	1·3	1·36 = 15·3
	out	1·4	1·4	1·5	,,	1·43	
1 wire	in	0·8	0·7	0·7	,,	0·73	0·74 = 8·32
	out	0·8	0·7	0·8	,,	0·76	

11582. Even by these rough experiments, the manner in which the effect is progressive in increase with the addition of each wire is well shewn, and the results with 1, 2, 3, 4, etc. wires shew that each wire would add to the effect its own full proportion, but that the conducting circuit is not proportionately increased in amount and therefore presents the first part of the series of gradual decrease of which the other end is shewn in the case of thick wires and copper cylinders (11568).

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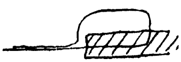
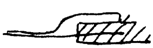
11583. Repeated the various experiments with the revolving bar magnets and wires (11322, etc.) using the thick wire galvanometer (11525) and thick copper wire communications of 0·2 of inch in thickness up to the two contact rings on the axis of the apparatus (11323). Copper wire of about 0·025 diameter was employed for the loop, etc. in and about the Magnets. The thick wire communications were together 13 feet in length. The contact and completeness of circuit was tried well in every case.



11584. The magnet and loop revolved together for any length of time gave no trace of an electric current (11324, 5).

11585. The wire moved over the magnet through 180° deflected the needle one way: moved in the other direction, the deflection of the needle was the other way. When the magnet was revolved 180°, the wire being still, the needle was also deflected. *Reverse* currents were given for the magnet and for the wire when they were revolved in the same direction (11338, 9, 42, 3).

11586. When the loop was altogether out of the magnet, and whatever its shape, if it revolved with the magnet and the same angular velocity—no current was produced (11331).

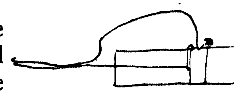


11587. The loop was now dissevered and the contact of the wires made at the centre of the magnet and at the equator, so that the equatorial disc or radius of the magnet was in the circuit. The effects described above (11584, 5) were exactly the same (11348, etc.).

11588. The wire contacts were now made at the pole and equator of the magnet—the effects were the same (11365).

11589. Now put on the copper equatorial ring of the magnet (11347), fixed a copper radius from the center to the ring, had

an axial wire going up to and bearing at the center against the radius, and the outer part of the loop in contact at the equatorial ring. So that the axial wire, the outer wire and the radial wire formed the circuit, but could move separately. The contact and communication was good (11583).



11590. The revolution of the axial wire alone did nothing (11367).

11591. The axial wire still (or moving), but the magnet, radial wire and outer wire revolving, did nothing (11368).

11592. The axial and the outer wire being still and the magnet and radial wire revolving, there was current (11371). When the radial wire was away and the body of the magnet used in its place, the same conditions and motions gave the same current.

11593. So the results with this galvanometer are precisely the same as with the fine wire Galvanometer of Rhumkorf.

11594. As to the *velocity of motion.* A slow motion gives a certain amount of swing—a quicker gives more, as before (11350, etc.). The needle can be held permanently deflected if the motion be continuous, and it is more deflected as the motion is more rapid, shewing in fact the effect of velocity of motion.

11595. Might perhaps find that a single, double, triple, etc. velocity would supply a good method of graduating such a galvanometer for proportionate forces.

11596. Ten revolutions of the bar magnets gave a swing deflection of 22°, but there was hardly time for ten revolutions well within the swing, because this pair of needles swing more quickly than those of the other galvanometer (   ).

11597. As to contact at the equator, found that for these low currents it ought to be given by a fair and full pressure. When that supplied, then five revolutions gave 20° or 22° of swing deflection, and that again and again.

11598. Now returned to experiments with loops (11552), the thick wire galvanometer and the horse shoe magnet, the latter being the small magnet, reduced by the keeper until it supplied a manageable power (11554, 66); and wished to see what a thick iron wire loop would do compared to copper. So had two as before (11552), the loop of one being copper wire and of the other iron, both 0·2 of an inch diameter. The iron was however longer

than the copper, perhaps $\frac{3}{4}$ of an inch, which should not have been, as it would be against it in conduction of the results.

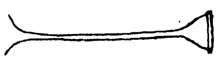
11599.

Copper loop	out	3·6	3·6	3·6 points	average	3·6	3·71 = 41°·7
0·2	in	3·7	4·0	3·8	”	”	3·83	
Iron loop	out	2·8	2·9	2·9	”	”	2·86	3·0 = 33°·7
0·2	in	3·25	3·25	3·0	”	”	3·16	

11600. The difference at going in and out appeared due to the position of the needle in the convolutions, and perhaps a little to some degree of torsion which was on the cocoon fibre, so that it tended to unwind one way more easily than to wind up in the other. The average is a good result.

11601. So the difference in copper and iron here nothing like so great as with thin wires (11573, 7), and this agrees with the effect of thickening the wires, for the copper looses in that respect *in relation to the conductors* far more rapidly than iron. I suppose that if the copper and the iron of the loop be still more thickened, they will at last arrive nearly at equality, the conductors remaining unchanged (11605).

11602. Arranged cylinders of copper and Iron and also bismuth for this kind of comparison. The cylinders were 5·5 inches long and 0·7 of an inch in diameter but could not now have the form of loops. They were therefore as before (11568), part of the copper



conductors forming the loop, but the metals to be compared forming the whole of that which passed across the magnetic axis and therefore across the chief amount of lines of force. No doubt the expanded part of the conductors did a little, but that would be the same for all.

11603.

Copper cylinder	in	3·6	3·2	3·0	3·2	3·2 points	average	3·24	3·17 = 35°·66
	out	3·0	3·0	3·2	3·2	”	”	3·10	

11604.

Iron cylinder	in	3·6	3·6	3·5	3·5	3·3	3·5	”	3·5	3·33 = 38·32
	out	3·3	3·0	3·2	3·2	3·1	3·2	”	3·166	

11605. So that, as was expected, the iron up to and equalled the copper (11601). It even surpassed it, but this might be for the same reason as that by which a smaller copper cylinder surpassed a larger one (11568), namely lateral conduction.

11606.

Bismuth cylinder	in	2·5	2·5	1·8	2·0	1·5	average 2·06	2·05 = 23°
	out	1·5	2·25	2·4	2·3	1·8	„ 2·05	

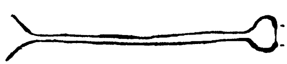
This relation of bismuth to iron and copper consists with the low conducting power of bismuth—if it could be compared as a wire of 0·04 diameter (11571) it would probably come out as almost nothing.

11607. This cylinder required great care in experiments with it. Long after one would have thought that the heat of the soldering had been equalized at the two ends, it presented (without the magnet of course) thermo currents or currents of that nature, being in circuit with the Galvanometer; and these currents, being feeble, were sometimes one way and sometimes the other—and seemed to be produced irregularly from time to time by some internal change, reminding one of the different periods of setting and the evolution of heat in Rose's experiments with fusible metal. It requires much care to avoid and pass by all these in the holding and handling the loop—the radiation from the hand to one end of the cylinder is enough to give a deflection at the Galvanometer.

11608*. Iron, tin, and lead, come near each other in the loop experiments with thin wire (11571–80) and in conducting power, so varied the experiment with them thus. Made the external part of the loop of the thick copper wire of 0·2 diameter, but the three inches which had to pass between the poles and across the dense part of the magnetic field were of wires 0·04 diameter and of the metals iron, tin and lead. Of course the effect of the whole loop is a mixed effect, but the bad conducting power of 7·5 inches of the thin wire is removed. The small horseshoe magnet, not reduced in power by the keeper, was employed.

Tin	in	3·4	3·4	3·4			average 3·4	3·3 = 37°·1
	out	3·2	3·2	3·2			„ 3·2	
Iron	in	3·6	3·3	3·0	3·2	3·2	„ 3·26	3·1 = 34°·8
	out	3·0	3·0	3·3	3·0	3·0	„ 3·06	
Lead	in	2·5	2·4	2·2			„ 2·36	2·26 = 25°·4
	out	2·0	2·2	2·3			„ 2·16	

11609. The proportions with Iron, tin and lead are almost identical with those obtained before (11576, 7, 9).

* [11608] 

11610. Now experimented with this thick wire galvanometer and the Discs as before (11540), using the large horseshoe magnet, and employed first the copper disc of 0·1 thickness and $2\frac{1}{2}$ inches diameter.

11611. Two revolutions with a soft equable bearing of the thick copper wire at the edge gave a deviation of 2 points or 22° on either side, according to the revolution. Hard bearing on the edge gave about 3 points or nearly 34°. The grooved edge disc was more convenient and gave a better result than the flat edged one.

11612. The revolution one way or the other produces a difference, because holding the wire as a pen and from above, one cannot make the same character of bearing on the edge–it pulls upon the hand by the friction in the one way and pushes against it in the other. Further, must take great care to make contact in exactly the same position each time, that the same radius may be active, whether the revolution is one way or another.

11613. Two revolutions make about 2·5 points with fair pressure = 28°.

11614. With the magnet away, the friction of the copper conductor against the copper disc sets the needle a little in one direction, always the same whichever way the disc rotates, i.e. direct.

11615. *Disc of copper*, 0·1 thick and 2·5 inches diameter (11541).

direct	2·0	1·9	1·9	1·9	average 1·92	1·84 = 20°·7 (11619)
reverse	1·6	1·9	1·8	1·8	„ 1·77	

11616. *Disc of Iron*, 0·2 thick and 2·5 inches diameter (11619).

direct	2·5	3·2	3·4	3·4	average 3·12	1·65 = 18°·5
reverse	0·3	0·3	0·0	0·4	„ 0·19	
			−0·25			

The effect of friction without the magnet was 1·4, 1·6, 1·5, 1·4 = 1·47 = 16°·53. By subtracting the reverse from the direct results and halving the result, we have $\frac{3{\cdot}12 - 0{\cdot}19}{2}$ = 1·46 = 16°·42, almost exactly the same result. So iron fairly represented here.

11617. So Iron is not above copper in the magnetic field. Its deficiency in conducting power is apparent, but being so short in the part which make[s] the galvanometer conducting circuit (only 1·25 inches), it rises up very nearly to copper. Here, as

the conductors of copper do not move, they will generate no current (11608) but act only as conductors.

11618. A *tin disc*, 0·1 thick and 2·5 inches diameter. Without the magnet, the friction of the copper conductor against the edge of the disc gave a deflection the contrary of the former and accordant with the reverse direction—hence the difference below. It was not much, being for ten revolutions about half a point.

direct	1·1	1·1	1·1	$1{\cdot}325 = 14^\circ{\cdot}9$
reverse	1·6	1·5	1·55	

I could not obtain many reverse effects, for the metal being soft, the disc would not hold on the axle screw.

11619. Two disc[s] of copper and iron on the axle at once, the copper one next the shoulder, which ensures good contact with the axle, then a disc of paper to insulate and then the iron disc screwed tight up. The iron disc was placed midway in the magnetic field—each was 0·2 thick and $2\frac{1}{2}$ diameter. The deflections were as follows:

Copper	direct	3·0	3·0			Average	3·0	$2{\cdot}75 = 30^\circ{\cdot}9$
	reverse	2·5	2·5			„	2·5	
Iron	direct	1·5	1·5	2·0	2·2	Average	1·8	$0{\cdot}9 = 10^\circ{\cdot}1$
	reverse	0·0	0·0	0·0		„	0·0	

Thinking the iron was affected by the copper or by not being in contact with the copper shoulder on the screw, the copper disc was removed and the iron disc put on alone—it then gave:

direct	1·5	1·5	1·8	1·8	average	1·65	$0{\cdot}85 = 9^\circ{\cdot}56$
reverse	0·0	0·2	0·0		„	0·06	

This is just what it was with the copper but only half what the iron gave before (11616), whereas the copper here is more than before (11615), but it is also twice the thickness. Try again.

11620. Had a disc made of box wood, 0·1 thick and 2·5 inches in diameter, with a copper center for the screw and connexion and a copper rim, and these were connected by a copper wire, soldered, of 0·05 in. thickness, to represent the action of a single radius. Without the magnet this disc gave me the usual small effect of copper friction at the edge. With the magnet (the large horse shoe) and continuous rotation it gave a good effect. With two rotations, for comparison with the former disc, it gave:

direct	1·5	1·6	1·6	1·5	Average	1·55	$1{\cdot}25 = 14^\circ$
reverse	1·2	0·8	0·8	1·0	„	0·95	

11621. So a disc with a single conducting radius gives 14° whilst a full or solid disc of copper gives only 20°·7 (11615). This shews how very greatly the parts lateral to the chief radius must conduct back again.

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11622. Experimented with discs—the large horse shoe magnet (11484) and the thick wire Galvanometer (11525) and connexions; and first employed the copper discs of different thicknesses—being all $2\frac{1}{2}$ inches in diameter.

11623. *Copper disc* 0·05 of inch thick—flat edge.

direct motion	1·7	2·0	2·0	2·1	average 1·95	1·85 = 20°·8
reverse motion	1·7	1·7	1·8	1·8	„ 1·75	

11624. *Copper disc* 0·1 of inch thick—flat edge.

direct	3·2	3·0	3·0	3·0	average 3·05	2·47 = 27°·8
reverse	2·0	2·0	1·8	1·8	„ 1·9	

11625. Have to take care of the center contact, that it is good as to pressure and position.

11626. *Copper disc* 0·2 *of inch thick*—flat edge.

direct	3·0	3·0	2·8	2·7	2·7	Average 2·84	2·33 = 26°·5
reverse	1·8	1·8	1·9	1·8	1·8	„ 1·82	

11627. So not better than the one of half the thickness—not quite so good—but better than that of a quarter thickness.

11628. *Iron disc of* 0·2 *inch* in thickness—flat edge.

direct	2·3	2·4	2·8	2·8	2·7	mean 2·6	1·25 = 14°
reverse	0·0	−0·2	−0·1	0·0	−0·2	„ −0·1	

11629. *Iron disc of* 0·05 *inch in thickness*—grooved edge.

direct	3·1	3·1	3·2	3·3	average 3·175	1·35 = 15°·4
reverse	0·3	−0·7	−0·7	−0·8	„ −0·475	

11630. So here in *iron*—the thick one is beyond and the maximum effect as before in *copper*.

11631. The difference in direct and reverse is of course due to the effect of friction—the Negative numbers in the reverse results are when the effect of friction overcame that of Magneto electric induction and so the needle went so far in the *direct* direction.

11632. Placed the two discs of *iron* and *copper* of 0·2 thick each on the axle at once, with paper between and the iron disc next the

shoulder on the axle—it also was placed mid distance between the poles so that the copper disc was on one side in that respect. Then observed the effect of the iron and copper discs in succession.

11633.

Iron 0·2	direct	2·0	2·2	2·1	2·4		average 2·175	1·075 = 12°·35
	reverse	0·3	0·0	0·0	−0·2		,, 0·025	
Copper 0·2	direct	2·3	2·1	2·0	1·8	1·8	average 2·0	1·78 = 20°
	reverse	1·7	1·8	1·3	1·4	1·2	,, 1·48	

Now this is below the same copper when alone (11626) and found upon examination that the central bearing had slipped out of the cavity and was only partially made. Corrected this and then observed:

11634.

Copper 0·2	direct	2·4	2·4	2·4	average 2·4	2·08 = 23°·4
	reverse	1·7	2·0	1·9	,, 1·86	

11635.

Iron again 0·2	direct	2·2	2·0	2·1	,, 2·1	1·02 = 11°·47
	reverse	0·0	− 0·2	0·0	,, − 0·06	

11636. The proportions between copper and iron are nearly as when those metals were observed separately (11626, 8) but the neighbouring mass appears to have an influence in lowering the amount (11643). Try this amongst coppers. It ought probably to be so, as the currents formed in them return in the lateral parts and so disturb the disposition of the magnetism and lines of force. If the currents did not return in the neighbouring parts, then probably no effect of this kind would be produced; and such is the case with thin wires, or thicker wires carried across lines of equal force.

11637. Probably this is the reason, in part or altogether, why a thick disc sinks in effect below a thin one (11627).

11638. *Bismuth disc* 0·2 in thickness—will not do—has far too much effect of friction, so as to send the needle out by swing 90° or more, and even by continued rotation holds it out 20° or 30°.

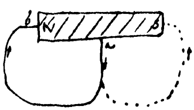
11639. In order to observe the effect of a copper edge put loosely on a disc as a conductor, I have had a hoop made out of thin copper foil just to fit the copper disc of 0·2 thickness; and cleaning it within and without well and the edge of the disc, I put it on,

wedging it up by introducing other pieces of thin copper foil, very clean. It gave the following results:

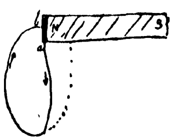
Direct	1·1	1·0	1·0	average	1·03	0·83 = 9°·34
Reverse	0·7	0·7	0·5	,,	0·63	

So the effect is reduced from 26°·5 to 9°·34 through want of contact. The result also probably partakes of the nature of that with the wooden disc and copper radius.

11640. To repeat and further complete some results with the revolving magnet, I arranged a cylindrical magnet so that I could revolve it and make contact of the Galvanometer wires either at the poles or at any part of its circumference, so as to compare different parts of the magnet together. The results are over leaf[1]—the degree of deflection was not observed, the direction of the current only being required.



11641. When the wire *a* touched the equator and the other wire *b* touched either pole, the current was in the same direction through *a* and therefore always (with the same rotation) from the axis of the magnet through its body to the equator. When the wire *a*, instead of touching at the equator, touched any where else on the round surface of the cylinder, the same result was obtained.



11642. When a disc of silver was fixed on the end of the magnet so as to rotate with it, and the wire *b* applied to its center as the axis and the wire *a* to its edge, the same direction of current was obtained as when *a* touched the surface of the magnet itself at the equator or elsewhere. So that the lines passing through the silver disc and the magnet are by the experimental results the same in direction and nature.

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11643. As to the action of lateral revolving discs and matter. The *copper disc* of 0·2 in. thickness was put up (   ), the central bearing made good and the effect of 2 rotations examined—results as follows:

direct revolution	1·8	1·8	1·8	average	1·8	2·43 points = 27°·3
reverse ,,	3·1	3·1	3·0	,,	3·06	

Then a paper disc and next the copper disc (flat edge) of 0·1 in. thickness.

[1] See pars. 11641, 42.

Disc of 0·2 examined again as to its action:

direct	1·8	1·8	1·6	1·8	average 1·75	$2{\cdot}13 = 24^{\circ}$
reverse	2·6	2·6	2·4	2·5	„ 2·52	

So its power seems decreased by the neighbouring revolving disc (   ).

Now took power of copper disc of 0·1, it being in company with the thicker disc:

direct	2·0	2·3	2·3	2·2	average 2·2	$2{\cdot}39 = 26^{\circ}{\cdot}9$
reverse	2·4	2·8	2·4	2·7	„ 2·575	

Then removed the thick disc and took observations with that of 0·1 thickness alone:

direct	2·1	2·2	2·0	average 2·1	$2{\cdot}36 = 26^{\circ}{\cdot}5$
reverse	2·6	2·7	2·6	„ 2·63	

So the general conclusion is not supported here by the thin disc (11636).

11644. *Bismuth disc* with the copper edge on (11639) for the deflection direction. It gives when still a permanent deflection, being a little warmer perhaps by handling than the copper axis, but when it was thus stationary, i.e. the needle, the direct rotation of the bismuth disc made the needle go east as the direct rotation of copper had done before with the same position of the magnet. The reverse revolution kept it stationary, the effect of the friction and of the induced current being contrary and equal. So it appears that the current is in the *same direction* for the rotation as in copper, iron, etc., but is very small because of small conducting power.

11645. The thing was very tender in its action; for touching the ring with a finger instantly gave a strong thermo current.

11646*. *Surrounding media*. The following arrangement was made for experiments on Air, Water, Alcohol and Camphine. A deep finger basin had the horseshoe magnet placed in as arranged. The conductors with the copper wire loop of 0·04 thickness (11572) were bent so that they could go into the finger basin, and when raised and lowered, make the loop pass through a limited distance of the magnetic field. A stop above and the bottom of the basin formed the limit to the action. The basin, magnet, etc. was blocked so that it could be removed and again restored to its place. The loop being either raised or lowered gave a deflection the amounts

* [11646]

of which either one way or the other from Zero are as below. Glass basin empty and therefore *air* the medium.

11647. Air—contact at the ends of the loop, so card placed between but not before. Camphine placed in the finger basin to fill it, so that first.

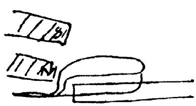
11648. *Camphine*—1·8, 1·6, 1·6, 1·6, 1·7, average 1·66=18°·6. The camphine taken out—all cleaned—things restored as to position and air the medium now.

11649. *Air*—1·6, 1·7, 1·5, 1·7, 1·7, average 1·64=18°·4. Water poured in.

11650. *Water*—1·6, 1·6, 1·5, 1·6, 1·6, 1·7, average 1·6=18°—cleaned out for air.

11651. *Air* again, 1·6, 1·6, 1·7, 1·6, 1·6, 1·6, average 1·61=18°·1. Alcohol in.

11652. Alcohol—1·7, 1·6, 1·6, 1·5, 1·6, 1·7, average 1·61=18°·1. May well consider these results as all alike.



11653. *Bar Magnet rotation.* The bar magnet with its complete loop (11583) was rotated, using the thick wire galvanometer (11525), and as before with no evolution of any current. Then the large horse shoe magnet was placed near one end of the magnet, so as to change greatly the disposition of the force; but revolution gave no effect as before. The horse shoe magnet was turned pole for pole, but still no effect. It was at these times within 1½ inches of the pole of the bar magnet. Shews how true the principles are for all forms of the line of magnetic force.

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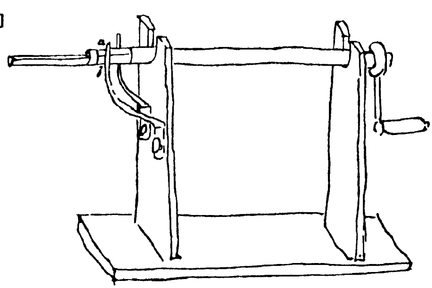
11654*. Began some experiments with revolving rings as investigating moving wires (11547). Certain of these rings have had their description given before. The apparatus to carry them is of the following kind. Two uprights on a wooden stand carry a wooden axle, at one end of which is the handle, and the other end projects and is shaped as in the figure. *a b* is a ring of copper fixed on to the axle but separated into two halves by cuts, of

* [11654]

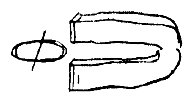


which one is seen in the horizontal position; and as two springs of copper connected with sockets, to receive the ends of the galvanometer wires, press against them on opposite sides, they form a commutator which changes the direction of any current at every half revolution.

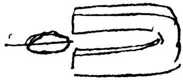
11655. The ring No. IV (11547), containing 49 inches of copper wire of 0·032 of an inch in thickness, disposed in 10 spirals of a ring 1·5 inches in diameter, was tied on to the flat projecting part of the axle and then its ends inserted in two notches in the wood under the half cylinders of the commutator, so as to make complete electric communication. Supposing such an apparatus to revolve; when the ring is horizontal the springs press against the notches or slits separating the two halves of the commutator, but as soon as motion has carried on the ring so as to make one part higher than the other, then the commutator comes into play. The motion of the upper parts of the spiral coincide[s] with that of the lower to make a certain current pass through the whole wire whilst lines of force are being intersected, and the action of the commutator causes these always to pass in the same direction to the Galvanometer. Hence accumulation of the effect.

11656. When this ring (11655) was revolved amongst the earth's lines of force, it sent the needle one way or the other according to the direction of the revolution—but the effect was weak. Four or five changes of rotation could swing the needle through 12° or 14°. The wire however is but short and the journey small, for the ring is only 1·5 inches in diameter.

11657. Put up the small horseshoe magnet so that its magnetic axis in the field of action was at right angles to the axis of rotation of the ring—in which case the revolving ring would intersect the curves favourably. The magnet was 2 inches from the axis of the ring and did not disturb the Galvanometer needle. On revolving the ring, the Galvanometer was well affected and could be permanently deflected 8° or 9°. By alternation of rotation obtain a large swing.



11658. When the magnet was advanced till about 1 inch from the axis of rotation—the permanent deflection was 14° or 15°. By four alternation[s] the swing of the needle was 90° or more. So can by such an apparatus well examine the possible affections of the lines.



11659. The axis of revolution was now placed end on to the magnet, also a favourable position. Had more effect than before, for the helix can go better up to and a little between the poles even.

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11660. Experimented with three loops (11598, 11552) of copper wire each containing 9·75 inches of wire and respectively of the follwg. diameters: 0·05–0·1–0·2 of an inch, so that the masses were as 1 : 4 and 16. The small horseshoe magnet reduced by the keeper was employed (11554, 66). The results were as follows:

Copper wire of 0·05 diameter.

In	1·3	1·25	1·4	1·4	1·5	1·5 points	average 1·39	1·42 = 16°
out	1·6	1·3	1·5	1·4	1·5	1·4 „	„ 1·45	

11661. The next was Copper wire of 0·1 diameter.

In	3·8	3·8	3·5	4·1	4·0	4·0	average 3·86	3·95 = 44°·4 (p. 2401)[1].
out	4·0	3·9	4·0	4·1	4·2	4·1	„ 4·05	

11662. Then the copper wire of 0·2 in diameter.

In	5·0	4·75	5·25	5·25	5·25	Average 5·1	5·1 = 57°·37
out	4·75	5	5·25	5·25	5·25	„ 5·1	

So here, though the masses were as 1, 4 and 16, the swing deflections were only as 16°, 44°·4, 57°·37, or as 1·0, 2·77, 3·58.

11663. A copper wire loop of 0·04 in. thickness, like the former in length (rather longer), was experimented with by the whole power of magnet and gave

In	1·75	1·75	2·0	1·75	1·8	Average 1·81	1·83 = 20°·58
out	1·75	2	1·8	1·8	1·9	„ 1·85	

11664. Have had an apparatus made, described already in (11654) –have extended the part to receive the rings–fixed on to the arm ring No. VI. (11547) containing 650 inches of copper wire 0·025 in. diameter in 64 spirals in a ring 3·3 inches diameter–and connected it with the *thick* wire Galvanometer (11525). Revolved it in the earth's lines of force; it affected the needle but not much. Less than I expected. Perhaps for this fine wire and such length, the other Galvanometer would be better.

11665. Put on Ring V. (11547) in which 120 inches of copper wire of 0·038 in. diameter–forming 19 spirals in a ring 2 inches in diameter. This gave a much better effect than the last, because the wire was thicker, the masses being as 14:6 nearly–the length was not $\frac{1}{5}$ of the former.

[1] I.e. par. 11852.